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PHILOSOPHICAL TRANSACTIONS.

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I. *On the Laws of the Tides on the Coasts of Ireland, as inferred from an extensive series of observations made in connection with the Ordnance Survey of Ireland.* *By* G. B. AIRY, *Esq., F.R.S., Astronomer Royal.*

Received November 30, 1844,—Read December 12, 1844.

IN the spring of 1842 I was informed by Colonel COLBY, R.E., Director of the Trigonometrical Survey, that in the operations of the Survey of Ireland it had become necessary to adopt a line of reference for the elevations ascertained in the running of various lines of level through the country; and that it was his intention to institute a series of observations of the height of the water in different states of the tide, in order to refer the levels to the mean height of the sea, or to its height at some definite phase of the tide. Colonel COLBY stated also that he was desirous that the observations should be made subservient to improvements in the theory of the tides, and requested my assistance in sketching a plan of observation which would be most likely to contribute to that end.

In reply, I made the following suggestions:—That great care should be taken in the accurate determination of time at every station, and that for this purpose the non-commissioned officer of the Royal Sappers and Miners who had the care of the observations at each station, should be entrusted with a pocket chronometer, and that an officer should, at least twice during the series of observations, visit every station, carrying, for comparison, an itinerant chronometer whose error on Greenwich time was accurately known from astronomical observations. That stations should be chosen on the eastern as well as on the western coast, in order to determine the difference of level, if any, between an open sea and a partially inclosed sea. That on the north-eastern coast, stations should be selected at smaller intermediate distances than at other parts of the coast, with the purpose of removing, if possible, the doubt which appears to exist as to the progress of the semidiurnal tide-wave through the North Channel. That, where practicable, several stations should be selected on each of the large rivers or estuaries, in order to ascertain the nature of the modification

which the tide-wave undergoes in passing up a contracted channel of comparatively small depth. That the series of observations should be so arranged, that, at every station, one complete tide (from high water to high water, or from low water to low water) should be completely observed on every day, its observations being made at small equidistant intervals. That supplementary observations, applying only to the neighbourhood of the low water or high water omitted in the observations of the complete tide, should also be made, for the development of the principal facts of diurnal tide. Finally, that the zeros of the tide-gauges should be connected with the principal lines of level, so that every observation should be referred to the same hydrostatic level.

These suggestions were adopted in their utmost extent by Colonel Colby. The collection of observations was placed in my hands in the winter of 1842. The whole number of observations exceeds two hundred thousand; the circumstances of place, simultaneity, extent of plan, and uniformity of plan, appear to give them extraordinary value; and extent of time alone appears wanting to render them the most important series of tide-observations that has ever been made.

Having under my immediate direction a large number of computers, employed at the Royal Observatory under the authority of the Lords Commissioners of the Treasury for the reduction of the Greenwich Lunar Observations, I requested the sanction of their Lordships for the employment of a part of this force on the reduction of these tide observations. With this request their Lordships were pleased to comply; and the investigations and results which I have now the honour to lay before the Royal Society are the fruit of this liberality.

The following is the order in which I propose to arrange the parts of this memoir:—

Section I.—Account of the stations, levellings, times, and methods of observation.

Section II.—Methods of extracting from the observations the times of high and low water; of supplying deficient times and heights; and of correcting the times first determined.

Section III.—Theory of diurnal tide as related to observations only; and deduction of the principal results for diurnal tide given immediately by these observations.

Section IV.—Theory of diurnal tide as referred to the actions of the sun and moon.

Section V.—Discussion of the height of apparent mean water, as deduced from the heights of high and low water only, corrected for diurnal tide; with reference to difference of station, and to variations of the phase of the moon, and of the declination of the moon.

Section VI.—Discussion of the range of the tide, and of semimenstrual inequality in height, apparent proportion of solar and lunar effects as shown by heights, and age of tide as shown by heights; from high water and from low water.

Section VII.—Establishment of each port, and progress of semidiurnal tide round the island.

Section VIII.—Semimenstrual inequality in time; proportion of solar and lunar

effects as shown by times, and apparent age of tide as shown by times; from high water and from low water.

Section IX.—Formation of the time of diurnal high water; progress of the diurnal tide-wave round the island; comparison of its progress and range with those of the semidiurnal tide.

Section X.—Method of expressing the height of the water, throughout every individual tide, by sines and cosines of arcs; and expressions in this form for every tide in the whole series of observations, except those at Courtown.

Section XI.—Discussion of the height of mean water deduced from the analysis of individual tides; with reference to difference of station, and to variations of the phase of the moon, and of the declination of the moon.

Section XII.—Discussion of range of tide, or coefficient of first arc in the analysis of individual tides; and of semimenstrual inequality in range, apparent proportion of solar and lunar effects, and age of tide as deduced from range.

Section XIII.—Establishment of each port, as deduced from the time of maximum of the first periodical term in the analysis of individual tides.

Section XIV.—Semimenstrual inequality in time, proportion of solar and lunar effects from times, and apparent age of tide as shown by times; deduced from the time of maximum of the first periodical term.

Section XV.—Comparison of the results as to mean height, range, semimenstrual inequality in height, age of tide obtained from height, establishment, semimenstrual inequality in time, and age of tide obtained from time, deduced from high and low waters only, in Sections V., VI., VII., VIII., with those deduced from the analysis of individual tides in Sections XI., XII., XIII., XIV.

Section XVI.—Remarks on the succeeding terms of the expressions for individual tides, as related to the magnitude of the tide, to the position on the sea-coast, to the position on the river, &c.; comparison with the terms given by the theory of waves; discussion of the quarto-diurnal tide.

Section XVII.—Separate discussion of the tidal observations at Courtown.

Section XVIII.—Examination into the question of tertio-diurnal tide.

Section I.—*Account of the Stations, Levellings, Times, and Methods of Observation.*

The following are the stations of observation:—

1. Kilbaha.—A small bay in the Shannon, on its north side, very near to the Loop Head; latitude 52° 34′, longitude 9° 52′ west of Greenwich. The gauge was a graduated post erected in the sea at a short distance from the pier; it was kept upright by large stones at its base, and by guys with large stones lashed to them.

2. Kilrush.—A small town on the north bank of the Shannon, about sixteen miles above Kilbaha; latitude 52° 38′, longitude 9° 29′. The gauge was a graduated scale nailed to the Revenue Pier.

3. Foynes Island.—An island in the Shannon, about fifteen miles above Kilrush; latitude 52° 37′, longitude 9° 9′. The gauge was a pole in the narrow channel on the south side of the island.

4. Limerick.—The tide-gauge was on the face of Mead's Quay, the lowest (on the course of the river) of the quays at Limerick; latitude 52° 38′, longitude 8° 39′.

5. Casleh Bay.—A small bay in the north side of Galway Bay, near its entrance; latitude 53° 14′, longitude 9° 34′. The tide-gauge was a pole in the water near the Coast-Guard Station.

6. Galway.—The tide-gauge was nailed to the pier, at the entrance to the New Dock; latitude 53° 17′, longitude 9° 2′.

7. Old Head.—A station on the south side of Clew Bay; latitude 53° 47′, longitude 9° 47′. The gauge was at first nailed to a small quay; it was afterwards fixed in the water.

8. Mullaghmore.—A station on the south side of Donegal Bay; latitude 54° 27′, longitude 8° 26′. The gauge was nailed to the south pier of a small port called Classybaun Harbour.

9. Buncrana.—A station on the east side of Lough Swilly, about ten miles from its mouth; latitude 55° 8′, longitude 7° 27′. The gauge was fixed in the water, opposite to a fortress called Ned's Point Battery.

10. Port Rush.—A small harbour near the entrance of Lough Foyle; latitude 55° 11′, longitude 6° 40′. The gauge was nailed to the northern pier.

11. Carrowkeel.—A station on the western side of Lough Foyle, about twelve miles from its mouth; latitude 55° 7′, longitude 7° 11′. The gauge was nailed to a large post permanently fixed in the water.

12. Ballycastle.—A small port opposite Rathlin Island; latitude 55° 12′, longitude 6° 14′. The gauge was a pole in the water.

13. Glenarm.—A small port between Ballycastle and Belfast; latitude 54° 56′, longitude 5° 56′. The gauge was nailed to the pier.

14. Donaghadee.—Latitude 54° 39′, longitude 5° 32′; near the south side of the entrance to Belfast Lough. The gauge was nailed to the pier.

15. Ardglass.—A small harbour opposite to the Isle of Man; latitude 54° 15′, longitude 5° 35′. The gauge was nailed to the pier.

16. Clogher Head.—A headland a few miles north of Drogheda; latitude 53° 48′, longitude 6° 14′. The observations were made at a small harbour called Port Oriel, on the north side of the Head, but very near to it. The gauge was nailed to the pier.

17. Kingstown.—The harbour on the south side of Dublin Bay; latitude 53° 18′, longitude 6° 9′. The gauge was nailed to the wharf, on the landward side of the harbour.

18. Courtown.—A small harbour; latitude 52° 38′, longitude 6° 14′. The gauge was nailed to the wall of a canal which forms the opening from a small river into the sea.

19. Dunmore East.—A small port on the west side of the entrance of Waterford Harbour (the confluence of the rivers Barrow and Suir); latitude 52° 9′, longitude 6° 59′. The gauge was nailed to the pier.

20. New Ross.—A town on the river Barrow; latitude 52° 24′, longitude 6° 56′. The gauge was nailed to the bridge.

21. Passage West.—This is the western side of the narrow channel by which the waters of the river Lee, at Cork, principally enter into the broad harbour of Cork. Its latitude is 51° 53′, longitude 8° 21′. The tide-gauge was nailed to the face of the steam-boat wharf.

22. Castle Townsend.—A small harbour at very nearly the southernmost point of Ireland; latitude 51° 30′, longitude 9° 20′. The gauge was a pole in the sea.

Of these stations it may be remarked that Kilbaha, Kilrush, Foynes Island, and Limerick, are four stations in succession on the same river; that Dunmore East and New Ross are on the same river; and that Port Rush and Carrowkeel are nearly in the same circumstances: Casleh Bay and Galway are in a relation nearly resembling this. Kilbaha, Casleh Bay, Old Head, Mullaghmore, Port Rush, Ballycastle, Glenarm, Donaghadee, Ardglass, Clogher Head, Kingstown, Courtown, Dunmore East, and Castle Townsend, are all on the open sea; Kilrush, Galway, Buncrana, Carrowkeel, and Passage West, are somewhat removed from it.

The zero of the tide-gauge at each place was referred as early as possible in the course of the observations to a permanent mark (usually a copper bolt driven into the face of a rock), and these marks were all connected by a system of levellings, and thus the zeros of the tide-gauges were all referred to the same mark, namely, a copper bolt fixed in the upper surface of the heelstone or hinge-stone of the gate of Buckingham Lock, Dublin. To give the means of recovering this important zero, in the event of its loss, the following references were made to several public buildings in or near Dublin:—

Building.	Position of the mark on the building.	Elevation of the mark above the bolt at Buckingham Lock.
		feet.
Four Courts	Pavement or floor of portico at principal entrance	+0·928
General Post-Office .	Pavement or floor of portico	+3·145
Bank of Ireland....	Pavement or floor of colonnade at principal entrance	+6·113
Custom House	Pavement or floor of portico at principal entrance.....	+2·195
Carlisle Bridge	Copper bolt driven horizontally into the stone-work of the battlement, 3·900 feet below the top of the battlement, and 2·840 feet above the centre of the road.....	+8·705
Queen's Bridge	Copper bolt driven horizontally into the stone-work of the battlement, 4·450 feet below the top of the battlement, and 1·580 foot above the centre of the road	+8·019
Trinity College	An arrow cut on the stone-work at the principal entrance in College Green, 3·300 feet above the surface of the ground.....	+4·026
Poolbeg Light House	A mark on the surface of the base-course, under the south window (the mark lower than the bolt at Buckingham Lock)	−0·020

It will be seen, in the sequel, that results are obtained relating to the relative levels of the surface of the sea at different places, which if established must be deemed highly important. In order, therefore, to show what confidence is due to the results of relative level, I think it necessary to give, for every instance in which the means of doing so exist, the amount of discrepancy between the results for the difference of level between the same stations when the space between them was traversed by different lines.

	Description of marks whose difference of level is ascertained, and courses of the lines of levelling.		Approximate length of line in miles.	Difference of level: By forward levelling.	Difference of level: By backward levelling.	Difference of backward and forward levelling.	Mean of backward and forward levelling.	Difference of the means by different lines.
				ft.	ft.	ft.	ft.	ft.
1.	Bolt in the front of the hotel, New Ross, above the bolt at Buckingham Lock, Dublin	By Monasterevan	101	10·698	11·826	1·128	11·262	0·171
		By Gorey	84	11·469	11·397	0·072	11·433	
2.	Mark at Lord Glengall's house, Cahir, below bolt in Monasterevan Church	By Borris-in-Ossery	72	48·203	48·482	0·279	48·342	0·418
		By Waterford	110	48·796	47·053	1·743	47·924	
3.	Bolt in Penrose Quay, Cork, below mark on William Street Bridge, Waterford	By Cahir	95	3·471	3·257	0·214	3·361	0·630
		By Dungarvan	85	3·973	4·015	0·042	3·994	
4.	Mark on Mead's Quay, Limerick, below mark at Court House, Borris-in-Ossery	By Nenagh	50	348·344	347·465	0·879	347·904	0··04
		By Holycross	68	347·661	347·940	0·279	347·800	
5.	Mark on Tralee Bridge, below mark on Holycross Bridge	By Limerick	105	272·575	273·262	0·687	272·918	0·264
		By Cahir and Mallow	122	272·389	272·920	0·531	272·654	
6.	Bolt in Loughrea Church, above the bolt at Buckingham Lock, Dublin	By Ballinasloe	108	268·558	268·993	0·435	268·775	0·135
		By Nenagh	130	268·762	268·519	0·243	268·640	
7.	Mark on Oranmore Bridge, above mark on Mead's Quay, Limerick	By Nenagh	76	7·573	7·981	0·408	7·777	0·336
		By Kilrush	109	8·341	7·886	0·455	8·113	
8.	Bolt in the Market House, Armagh, above the bolt at Buckingham Lock	By Dundalk and Newry	82	146·771	147·146	0·375	146·958	0·147
		By Navan, Cavan, Newtown Butler and Monaghan	118	147·061	146·561	0·500	146·811	
9.	Bolt in the front of Commercial Buildings, Belfast, below bolt on Sugar Island Bridge, Newry	By Armagh, Portadown, and Lisburn	58	5·477	5·489	0·012	5·483	0·056
		By Downpatrick and Newtown Ards	62	4·970	6·109	1·139	5·539	
10.	Bolt in Port Rush Pier, below bolt in Commercial Buildings, Belfast	By Glenarm and Ballycastle	83	6·327	5·086	1·241	5·706	0·908
		By Antrim and Coleraine	62	6·334	6·893	0·559	6·614	
11.	Bolt in Castlebar Gaol, below mark at Kinnegad	By Longford, Ballysadare and Ballina	140	98·589	98·341	0·248	98·465	0·487
		By Ballinasloe and Tuam	122	98·795	99·110	0·315	98·952	
12.	Bolt in Commercial Buildings, Belfast, below the bolt at Buckingham Lock	By Newtown Butler and Armagh	156	0·486	1·087	0·601	0·786	0·091
		By Newry, Downpatrick and Newtown Ards	125	0·269	1·122	0·853	0·695	
13.	Mark at Londonderry Bridge, above bolt at Buckingham Lock	By Newtown Butler, Monaghan and Strabane	168	2·828	2·351	0·477	2·589	1·058
		By lines round the east and north coasts	243	.....	.....	.....	3·647	
14.	Mark at Cliffony, near Mullaghmore, above bolt at Buckingham Lock	By Newtown Butler and Ballyshannon	139	93·998	93·117	0·881	93·557	0·140
		By Kinnegad and Ballysadare	145	92·520	94·314	1·794	93·417	

On these results, the following remarks are made by Colonel Colby.

The discordance in No. 10 is unusally great; but as it appears from an examination of the levelling that the error in discordance is of gradual accumulation and does not arise from any one mistake, the mean of the two results is adopted for Port Rush, and a part of the difference between this mean and the result on either line, proportional to the distance of any point on that line from Belfast, is adopted as a correction for the apparent elevation of such point.

In No. 13, second line, the comparison of forward and backward levelling is omitted, the principal part (namely that from Dublin to Port Rush) being contained in Nos. 10 and 12. The mean of the two results for Londonderry was adopted; upon this depend the zeros of Carrowkeel and Buncrana.

The station at Limerick being important, its relation of height to that at Dublin was ascertained by four different lines. The following Table contains the results of

these levellings. The first result, or that by the shortest line, was adopted in the reduction of the tide-observations.

Depression of the Bench-mark at Mead's Quay, Limerick, below the bolt at Buckingham Lock, Dublin.

Line along which the levels were carried.	Approximate length in miles.	Depression by forward levelling.	Depression by back levelling.	Mean of results forward and back.
		feet.	feet.	feet.
From Dublin by Monasterevan and Borris-in-Ossory; the difference of level from Borris-in-Ossory to Limerick being the mean of those found by Nenagh and by Holycross	127	0·629	0·651	0·640
From Dublin by Cork, Mallow, and Tralee; the difference of level from Dublin to Cork being the mean of those found by Monasterevan, Borris-in-Ossory, Cahir and Mallow, and by New Ross, Waterford and Dungarvan	317	0·032	0·744	0 388
From Dublin by Gorey, New Ross, Waterford, Mallow and Tralee: the difference of level from Waterford to Mallow being the mean of those by Cahir and by Cork	312	0·249	−0·350	−0·050
From Dublin by Ballinasloe, Oranmore, Liscanor and Kilrush	233	0·899	0·889	0·894

In order to give the means of verifying the principal results of the tide-observations at any future time, I subjoin a statement of the positions of the bench-marks at the different tide stations, and of their difference in elevation from the bolt at Buckingham Lock, Dublin.

Tidal Station.	Description of the permanent mark.	Elevation of the mark above the bolt at Buckingham Lock.
		feet.
Kilbaha	Top of copper bolt driven vertically into one of the facing-stones of the pier	− 2·814
Kilrush	Top of copper bolt driven vertically into one of the facing-stones of the pier	− 1·974
Foynes Island	Top of copper bolt driven vertically into the solid rock	− 2·920
Limerick	Copper bolt driven horizontally into one of the facing-stones at Russell's Quay	− 0·850
Casleh Bay	Top of copper bolt driven vertically into the solid rock, close to the Coast-Guard Watch House	+ 0·004
Galway	Copper bolt driven horizontally into one of the facing-stones at the entrance to the New Dock	− 3·000
Old Head	Copper bolt driven vertically into one of the facing-stones of the quay	− 0·870
Mullaghmore	Copper bolt driven vertically into one of the facing-stones of the south pier	− 1·749
Buncrana	Copper bolt driven horizontally into the scarpwall of Ned's Point Battery	+18·203
Port Rush	Copper bolt driven vertically into one of the facing-stones of the quay	− 6·900
Carrowkeel	Copper bolt in the wall of the Police barrack	+24·820
Ballycastle	Copper bolt driven vertically into one of the stones of the quay	− 2·268
Glenarm	Copper bolt driven into the solid rock on which the pier is built	− 9·388
Donaghadee	Copper bolt driven vertically into one of the facing-stones of the quay	+ 0·321
Ardglass	Copper bolt driven vertically into one of the facing-stones of the pier, near the steps	+ 0·838
Clogher Head	Mark on one of the facing-stones of the pier	+ 2·567
Kingstown	Top of copper bolt driven vertically into one of the facing-stones of the pier	− 2·796
Courtown	Top of copper bolt driven vertically into one of the facing-stones of the entrance to the harbour	− 4·459
Dunmore East	Top of copper bolt driven vertically into one of the facing-stones of the pier	− 3·711
New Ross	Mark in one of the facing-stones of the quay	− 4·178
Passage West	Top of copper bolt driven vertically into one of the coping-stones at the edge of the pier	− 6·463
Castle Townsend	Top of iron bolt driven vertically into the rock in which the Coast-Guard signal-staff is secured	+10·739

The results for height in the subsequent sections of this paper are all referred to a point thirty feet below the bolt at Buckingham Lock, Dublin.

At each of the stations the course of observation was as follows:—The observer adopted, as the tide which was to be completely observed, either the interval from high water to high water, or that from low water to low water, according to the convenience of the hours. Thus, having begun, for instance, with commencing a tide at the morning high water, when the high water occurred at convenient hours both in the morning and the evening; as the tides in the succession of days fell later and later every day, the termination of the tide at last fell inconveniently late in the evening, and the observer then began his observations about six hours earlier in the morning, so as to commence with low water and to terminate with low water. After a time it became necessary, in consequence of the evening low water occurring inconveniently late, to commence again with high water; and thus there was in every few days a change in the arrangement of observations.

The observations were generally commenced about half an hour before the commencing high water or low water, and were generally continued about half an hour after the terminating high water or low water. Thus, of the four principal phases which occur in each day (two high waters and two low waters), three were effectually observed in the day series of observations. As there were at each station at least two observers, one of these persons made observations for an hour or more in the night, partly before and partly after the remaining high water or low water; and thus all the high waters and low waters were observed. This system had the advantage of giving all the phenomena of diurnal tide, and giving one semidiurnal tide completely observed in each day, with little distress to the observer. Its only disadvantage is, that the observations at different stations do not always apply to the same portions of corresponding tides; but there appears to be no method of securing this precise correspondence of observations except by incessant observations day and night, or by self-registering tide-gauges. Each observer registered the height of the water on his tide-pole at every five minutes by his watch.

The watches were for the most part chronometers or lever watches. An officer visited each station at least three times, and the greater number of the stations four times, carrying a good pocket chronometer whose error on Greenwich time was known. Two itinerant chronometers were thus employed. The error of each of the observers' watches was afterwards computed for every day of observation from these comparisons, and this error was applied to form the corrected Greenwich time of every observation, in a column purposely left in the sheets of observations.

At two stations only, Ballycastle and Glenarm, the means for registering the time proved imperfect. At the former, in consequence of the failure of the watch, the time was taken from the town-clock, and corrected for the longitude of the place; it is supposed that this time may be sometimes ten minutes in error. I much regret that the extraordinary phenomena of the tides at Ballycastle are thus developed with

less certainty than could be desired; at the same time I have no hesitation in expressing my belief, that the credit of the results hereafter to be given is not sensibly injured by this circumstance. At Glenarm, from a similar cause, it became necessary to refer to the post-office clock; but the observations do not appear to have suffered materially.

The observations began about June 22, and were discontinued about August 22.

Section II.—*Methods of extracting from the observations the times of high and low water; of supplying deficient times and heights; and of correcting the times first determined.*

The determination of the height of the water, at high or low water, from the observations, was a matter of no difficulty. In two or three instances of low water, when the water had dropped below the zero of the tide-gauge, the observations were incomplete till it again rose to the zero; in these, the observations were supplied by comparison with other low waters which had been completely observed.

The determination of the time was far more difficult. The examination of these observations has made me very distrustful of the results which have been deduced from observations of time only. The difficulty of fixing on the precise time of high or low water will appear from this statement, that sometimes twenty or twenty-four successive observations (occupying $1^h\ 40^m$, or 2^h) are registered with the same decimal of a foot for the height. The most perplexing case is that where the change of height, in respect to change of time, follows or may follow different laws before and after the principal phase. Thus at Limerick, after low water, the water sometimes rises as much in ten minutes as it had previously dropped in two hours; it therefore appears right here, if several successive observations about low water are registered at the same decimal of a foot, to suppose that the real low water is little before the last of those observations. At some other stations this circumstance does not happen uniformly; and then, when it does happen, it becomes difficult to say whether there is a difference of law before and after the low water (in which case the real low water ought to be taken nearer to the last observation), or whether the surface of the water at the last observations on the same division has been depressed by accident (in which case the real low water ought to be taken nearer to the first observations). I will not undertake to say that, in marking off the times of high and low water, I have followed a uniform method in these difficulties; but I have certainly followed a uniform plan for each station; and this is all that is important.

Occasionally, though rarely, observations of high and low water were interrupted by the roughness of the sea and other accidents. It was highly desirable to supply these, because (as will be seen in the next section) differences of the heights and of the times to the fourth order were to be taken, and thus the omission of one height or time would entail the loss of five results in these differences of the 4th order. The following is the process by which they were supplied. It very soon became

evident to all who inspected the collected heights at high and low water, that irregularities in the heights at any one station were sensible with no important difference of magnitude at the neighbouring stations. This will be abundantly shown in Section XI. On this assumption, a comparison of the height or time at one station with that at each of the neighbouring stations, for a few tides near to that at which the observation was deficient, would give the means of supplying the omitted height or time. But it was necessary to bear in mind that all the observations were affected by diurnal tide, and that the diurnal tide might vary sufficiently from port to port to render it unsafe to use comparisons of evening tides for the correction of morning tides, &c. The process adopted therefore was the following:—The results, both for times and for heights, were divided into four groups. One comprehended the high waters which next followed the moon's transit; these were called High Waters of the First Division. Another comprehended the low waters which next followed those high waters; these were called Low Waters of the First Division. The remaining high waters and low waters were called respectively High Waters and Low Waters of the Second Division. Each of these groups was treated separately. When a height or time of high or low water at any station was to be supplied, the observed height, &c. at that station was compared with the mean of the observed heights, &c. for at least two neighbouring stations, in at least two tides preceding and two tides following, in the same group; and the mean difference thus found was applied to the mean of the observed heights, &c. at the stations compared, on that day for which the tide was deficient. I have no doubt that the results thus supplied are sensibly as accurate as those which were actually observed.

On consideration of the difficulty of determining the times of high and low water, which has been already explained, it appeared necessary to endeavour to smooth down some of their irregularities, without at the same time endangering the conclusions as affected by difference of diurnal tides and of semidiurnal tides at the different stations. The following is the method employed:—Each of the groups already mentioned was separated into four subdivisions, determined by the proximity of stations. One included Kilbaha, Kilrush, Foynes Island, Limerick, Casleh Bay, Galway, and Old Head. The second included Mullaghmore, Buncrana, Port Rush, Carrowkeel, and Ballycastle. The third contained Glenarm, Donaghadee, Ardglass, Clogher Head, and Kingstown. The fourth contained Dunmore East, New Ross, Passage West, and Castle Townsend. [Courtown was omitted, because, as will be hereafter seen, no times of high or low water could be fixed for it.] Then each subdivision was treated separately. For each tide the mean of the times for all the different stations was taken (Buncrana, Ballycastle, and Glenarm, being excepted; as, from the small range of tide at these places, the determinations were more uncertain than at others). Then for every station (including those already named) the difference of the time from the mean of times was formed. Thus, for any one station, a difference from mean was obtained for each day. Let these differences for successive days be called

D_1, D_2, D_3, D_4, D_5, &c. Then the means of the adjacent numbers were taken,

$$\frac{D_1+D_2}{2},\ \frac{D_2+D_3}{2},\ \frac{D_3+D_4}{2},\ \frac{D_4+D_5}{2},\ \&c.;$$

and the means of the numbers in this series were taken, forming

$$\frac{D_1+2D_2+D_3}{4},\ \frac{D_2+2D_3+D_4}{4},\ \frac{D_3+2D_4+D_5}{4},\ \&c.$$

Then the number $\frac{D_1+2D_2+D_3}{4}$ was considered to be the just difference from mean for the second day in the series: it was applied to the mean of times for that day, and gave the adopted time for high or low water for that day, at the station under consideration; and so for the succeeding days. In regard to the legitimacy of this process, it is to be observed that it does not suppress the inequalities affecting, in different degrees at different stations, the semidiurnal or diurnal tide, provided the period of such inequalities is of several days. Nor does it suppress any accidental inequality which affects the whole tide-wave coming from the Atlantic upon a large extent of coast. The only failure is, that, as

$$\frac{D_1+2D_2+D_3}{4}=D_2+\frac{D_1-2D_2+D_3}{4}=D_2+\frac{1}{4}(\text{2nd difference});$$

when the second difference of D is large, an error is introduced. So long as the tides at the different stations follow anything like similar laws, there is no fear that this error will be perceptible. The only place where there is any probability that it can become sensible is Ballycastle; and here it will be very far below the irregularities of observation.

Section III.—*Theory of diurnal tide as related to observations only; and deduction of the principal results for diurnal tide given immediately by these observations.*

The remarks with which I shall immediately proceed apply equally to times and to heights, and equally to high waters and to low waters; but, to avoid unnecessary repetitions, I shall speak only of heights at high water.

Suppose then that, for any station, the heights at high water, both of the First Division and of the Second Division, have been collected and intermingled in the order of times. It is evident that the diurnal tide at any one of these heights will be found approximately by taking half the excess of that height above the mean of the two heights immediately preceding and immediately following. The number thus found will, however, be in error by one-fourth of the second difference of the semidiurnal tide. This error may be eliminated, leaving only an error depending on fourth differences, by taking half the algebraical excess of that apparent diurnal tide above the mean of the diurnal tides next to it.

The process may however be put in the following algebraical form:—Suppose the successive high waters to be affected with inequalities represented by $a.\cos\overline{n-3}.\ell$,

$a.\cos\overline{n-2}.\theta$, $a.\cos\overline{n-1}.\theta$, $a.\cos n\theta$, $a.\cos\overline{n+1}.\theta$, $a.\cos\overline{n+2}.\theta$, &c., where n increases by unity for each successive high water. If we take the 4th, the 8th, the 12th, &c. differences of these numbers, we shall have for the differences standing opposite to $a.\cos n\theta$,

$$a.\cos n\theta \times 16 \sin^4\frac{\theta}{2},$$

$$a.\cos n\theta \times 256 \sin^8\frac{\theta}{2},$$

$$a.\cos n\theta \times 4096 \sin^{12}\frac{\theta}{2}.$$

Now if the inequality occupies many tides in going through its changes, that is, if θ is small, the powers of $\sin\frac{\theta}{2}$ will be very small, and these differences will therefore become smaller and smaller till they are nearly insensible. There is one value of θ, however, for which they do not become smaller, namely, that which makes $\sin\frac{\theta}{2}$ nearly $=1$, or θ nearly $=180°$, or in which the successive numbers $a.\cos\overline{n-1}.\theta$, $a.\cos n\theta$, $a.\cos\overline{n+1}.\theta$, &c. have nearly equal magnitudes with a change of sign at every step. It is evident that this is the case of diurnal tides. Consequently, on taking the successive differences in this manner, the diurnal tide will ultimately be the only inequality sensible.

If then we stop at the fourth differences, we may say that the diurnal tide $=\frac{\text{fourth difference}}{16\sin^4\frac{\theta}{2}}$: if we stop at the eighth difference, the diurnal tide $=\frac{\text{eighth difference}}{256\sin^8\frac{\theta}{2}}$; and so on, the expressions becoming more accurate as we advance further in the order of differences. Remarking, however, that the diurnal tide goes through all its changes in not fewer than 57 high waters, and that θ therefore differs from 180° by little more than 6°, or that $\sin\frac{\theta}{2}=\cos 3°$ nearly $=1-\frac{1}{800}$ nearly, we may consider the powers of $\sin\frac{\theta}{2}$ as equal to unity; and thus we have

$$\text{Diurnal tide} = \frac{1}{16}\times \text{4th difference},$$

$$\text{or} = \frac{1}{256}\times \text{8th difference},$$

$$\text{&c.}$$

The first of these formulæ was used throughout, both for heights and for times, and at both high and low waters.

Let us now consider the relation between the diurnal tide in height and that in time. Let θ be an angle increasing uniformly with the time, and increasing by 360° in a tidal day, its origin being the time of high water in the semidiurnal tide. Let a

be the diurnal tide at the first high water, b that at the first low water, c the semi-range of the semidiurnal tide. And suppose a and b to be so much smaller than c that their squares, &c. may be neglected. The height of the water above its mean height, on the law of elevation usually assumed, will be $a.\cos\theta+b.\sin\theta+c.\cos 2\theta$. This quantity will be maximum or minimum, or there will be high water or low water, when $-a.\sin\theta+b.\cos\theta-2c.\sin 2\theta=0$. The first approximation to the value of θ will be obtained by considering the large term only: from this we find $2c.\sin 2\theta=0$, from which $\theta=0$, or $=\frac{\pi}{2}$, or $=\pi$, or $=\frac{3\pi}{2}$, nearly. Substituting these values successively in the small terms, and supposing them liable to a correction x in the large term, we have,

For the first high water, $+b-2c.\sin(0+2x)=0$; or, nearly,

$$b-4cx=0;\ \text{whence}\ x=\frac{b}{4c},\ \text{and}\ \theta=0+x=\frac{b}{4c}.$$

For the first low water, $-a-2c.\sin(\pi+2x)=0$; or, nearly,

$$-a+4cx=0;\ \text{whence}\ x=\frac{a}{4c},\ \text{and}\ \theta=\frac{\pi}{2}+x=\frac{\pi}{2}+\frac{a}{4c}.$$

For the second high water, $-b-2c.\sin(2\pi+2x)=0$; or, nearly,

$$-b-4cx=0;\ \text{whence}\ x=\frac{-b}{4c},\ \text{and}\ \theta=\pi+x=\pi-\frac{b}{4c}.$$

For the second low water, $+a-2c.\sin(3\pi+2x)=0$; or, nearly,

$$+a+4cx=0;\ \text{whence}\ x=\frac{-a}{4c},\ \text{and}\ \theta=\frac{3\pi}{2}-\frac{a}{4c}.$$

It appears therefore that the diurnal equation in time at the High waters of the First Division has the same sign as, and is a certain multiple of, the diurnal equation in height at the Low waters of the First Division; and that the diurnal equation in time at the Low waters of the First Division has the same relation to the diurnal equation in height at the High waters of the First Division; and similarly for those of the Second Division. The factor by which the diurnal tide at low water in height is converted into diurnal tide at high water in arc is $\frac{1}{4c}$; and, observing that π in arc corresponds to about $12^h\ 24^m$ in time, the factor for converting diurnal tide at low water in height into diurnal tide at high water in minutes of time is $\frac{744}{4\pi.c}=\frac{186}{\pi.c}$; and that by which the diurnal tide at low water in minutes of time is converted into diurnal tide at high water in height is $\frac{\pi.c}{186}$. The same factor applies for converting diurnal tide at high water in minutes of time into diurnal tide at low water in height. But the high and low waters of the First Division must be used together, and the high and low waters of the Second Division must be used together.

This theory cannot be expected to apply with accuracy to any place far from the

sea (as Limerick or New Ross), where the law of the height of semidiurnal tide, as depending on the time, differs sensibly from that of $\cos 2\theta$.

Upon investigating the magnitude of the diurnal tides, by the method detailed a short time since, it appears that, at most stations, the diurnal tide in height was given with great regularity; but that, at the greater number of stations, the diurnal tide in time was not very regular. In order to compare the diurnal tides by means of the theory above, as well as for the purpose of ascertaining their magnitudes with some accuracy, it was necessary so to combine them that a mean of many determinations could be made available. This was done in the following manner:—

First, it is to be remarked that in this and all the following investigations the high and low waters of the first division only are used; these being evidently sufficient for the complete solution of any problem of diurnal tides.

Next, it is well known, or may be anticipated from the investigations of the next section, that on examining successively the diurnal tides at high water (first division) on successive days, they increase, diminish, change sign, and increase and diminish with the changed sign, in nearly the same manner as the sine of an arc increasing proportionally to the time; and that the same remark applies to the diurnal tides at low water.

The first thing to be done in investigation was therefore to ascertain when the diurnal tide vanishes. This was done by taking the five diurnal tides nearest to the estimated place of evanescence and combining them by the method of minimum squares, on the supposition that the diurnal tide ought there to alter by uniform steps; an assumption sensibly correct.

The next thing was, to take the mean of all the diurnal tides between two vanishing points. Supposing them to be expressed by the law $a.\sin\theta$, the mean of all these values is $\frac{\text{Sum of the values of } a\sin\theta}{\text{Number of values}}$, which is approximately expressed by $\frac{1}{\pi}a\int_{\theta}\sin\theta=\frac{2a}{\pi}$, and hence the coefficient a, or the maximum diurnal tide, must $=\frac{\pi}{2}\times$ the mean of the diurnal tides between two vanishing points.

The following results have been obtained by these methods:—

Diurnal Tide in Height at High Water. First Division.

ate time aha.	Kilbaha.	Kilrush.	Foynes Island.	Lime-rick.	Casleh Bay.	Galway.	Old Head.	Mul-laghmore.	Bun-crana.	Port Rush.	Carrow-keel.	Bally-castle.	Glen-arm.	Donagh-adee.	Ard-glass.	Clogher Head.	Kings-town.	Dun-more East.	New Ross.	Passage West.
hrs.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
17	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
18	.....	−0·23	.....	.....	−0·22	.....	−0·72	−1·05	−0·64	−0·72	−0·51	.....	−0·76	−0·72	−0·78	−0·75	−0·63	−0·24	−0·21	.....
19	−0·40	−0·29	.....	.....	−0·26	−0·30	−0·64	−0·73	−0·86	−0·97	−0·78	.....	−0·83	−0·76	−0·78	−0·63	−0·59	−0·19	−0·18	−0·16
19	−0·37	−0·26	−0·41	−0·30	−0·31	−0·34	−0·61	−0·99	−0·84	−0·81	−0·74	.....	−0·83	−0·85	−0·76	−0·69	−0·61	−0·13	0·00	−0·16
20	−0·53	−0·50	−0·47	−0·46	−0·49	−0·42	−0·68	−0·99	−0·71	−0·58	−0·68	.....	−0·72	−0·76	−0·74	−0·71	−0·63	−0·13	−0·16	−0·14
21	−0·30	−0·40	−0·41	−0·42	−0·38	−0·40	−0·60	−0·91	−0·62	−0·56	−0·66	.....	−0·66	−0·71	−0·71	−0·73	−0·72	−0·14	−0·17	−0·17
22	−0·30	−0·30	−0·36	−0·26	−0·34	−0·38	−0·40	−1·16	−0·70	−0·57	−0·63	−0·74	−0·65	−0·73	−0·79	−0·78	−0·75	−0·14	−0·18	−0·10
22	−0·18	−0·24	−0·26	−0·24	−0·23	−0·27	−0·18	−0·88	−0·57	−0·49	−0·59	−0·53	−0·35	−0·54	−0·64	−0·67	−0·55	−0·09	−0·12	−0·02
23	−0·09	−0·16	−0·13	−0·24	−0·12	−0·11	−0·14	−0·61	−0·64	−0·56	−0·67	−0·48	−0·32	−0·36	−0·48	−0·52	−0·39	−0·18	−0·12	−0·07
0	−0·01	−0·04	−0·03	−0·28	−0·02	−0·23	−0·20	−0·24	−0·56	−0·54	−0·61	−0·33	−0·30	−0·22	−0·32	−0·34	−0·25	−0·14	−0·15	−0·09
1	+0·23	+0·04	+0·05	−0·19	+0·09	−0·13	+0·09	−0·38	−0·34	−0·28	−0·18	+0·06	+0·12	+0·11	+0·05	+0·02	+0·08	−0·25	−0·13	+0·01
2	+0·26	+0·17	+0·09	−0·02	+0·20	+0·03	+0·07	−0·24	−0·28	−0·22	−0·06	+0·08	+0·16	+0·16	+0·19	+0·17	+0·16	−0·20	−0·06	+0·05
3	+0·21	−0·06	+0·10	+0·25	+0·23	+0·11	+0·02	+0·05	−0·16	+0·01	−0·05	+0·16	+0·12	+0·06	+0·14	+0·17	+0·15	−0·04	+0·06	+0·11
4	+0·11	−0·12	+0·12	+0·28	+0·16	+0·19	+0·28	+0·41	+0·16	+0·13	+0·05	+0·32	+0·27	+0·22	+0·21	+0·21	+0·20	+0·01	+0·06	−0·03
4	+0·07	+0·13	+0·22	+0·46	+0·02	+0·12	+0·21	+0·41	+0·44	+0·46	+0·32	+0·51	+0·38	+0·41	+0·37	+0·17	+0·28	+0·11	+0·08	−0·03
5	+0·24	+0·28	+0·42	+0·64	+0·14	+0·18	+0·20	+0·31	+0·52	+0·53	+0·56	+0·51	+0·56	+0·54	+0·51	+0·47	+0·45	+0·18	+0·31	+0·02
6	+0·37	+0·31	+0·47	+0·72	+0·46	+0·32	+0·55	+0·76	+0·74	+0·57	+0·65	+0·61	+0·79	+0·72	+0·27	+0·58	+0·51	+0·11	+0·24	+0·11
7	+0·29	+0·21	+0·22	+0·23	+0·22	+0·20	+0·42	+0·69	+0·51	+0·27	+0·25	+0·26	+0·59	+0·50	+0·34	+0·52	+0·39	+0·03	+0·11	+0·06
8	+0·16	+0·24	+0·23	+0·22	+0·10	+0·21	+0·34	+0·84	+0·56	+0·43	+0·37	+0·40	+0·79	+0·67	+0·41	+0·56	+0·43	+0·11	+0·22	+0·16
9	+0·26	+0·31	+0·28	+0·16	+0·27	+0·20	+0·47	+0·83	+0·45	+0·36	+0·39	+0·41	+0·98	+0·88	+0·69	+0·75	+0·56	+0·19	+0·28	+0·16
9	+0·11	+0·12	+0·09	−0·02	+0·04	−0·09	+0·29	+0·82	+0·50	+0·40	+0·45	+0·44	+0·80	+0·71	+0·56	+0·66	+0·42	+0·11	+0·17	+0·11
10	+0·04	+0·09	+0·06	−0·05	−0·05	−0·08	+0·21	+0·93	+0·44	+0·28	+0·25	+0·27	+0·69	+0·62	+0·49	+0·52	+0·37	+0·16	+0·12	−0·06
11	+0·03	−0·01	−0·08	+0·03	−0·10	−0·07	+0·15	+0·62	+0·34	+0·29	+0·16	+0·27	+0·56	+0·49	+0·48	+0·33	+0·38	+0·13	−0·03	−0·01
12	−0·12	−0·18	−0·23	−0·17	−0·24	−0·29	−0·20	+0·27	+0·27	+0·20	+0·02	+0·29	+0·27	+0·23	+0·21	+0·28	+0·11	+0·13	+0·01	−0·01
13	−0·27	−0·20	−0·28	−0·47	−0·30	−0·37	−0·30	−0·01	+0·02	−0·02	−0·05	+0·01	−0·04	−0·09	+0·08	−0·04	−0·09	−0·01	−0·02	−0·06
14	−0·33	−0·32	−0·35	−0·54	−0·24	−0·38	−0·27	−0·32	−0·29	−0·23	−0·28	−0·25	−0·23	−0·24	−0·22	−0·26	−0·23	−0·09	−0·12	−0·12
15	−0·37	−0·18	−0·41	−0·40	−0·32	−0·31	−0·37	−0·52	−0·44	−0·41	−0·48	−0·38	−0·28	−0·28	−0·42	−0·36	−0·29	−0·16	−0·31	−0·18
16	−0·41	−0·36	−0·41	−0·38	−0·39	−0·41	−0·57	−0·74	−0·61	−0·55	−0·64	−0·50	−0·34	−0·44	−0·50	−0·50	−0·39	−0·20	−0·35	−0·19
17	−0·42	−0·34	−0·39	−0·41	−0·41	−0·41	−0·55	−0·79	−0·68	−0·62	−0·62	−0·58	−0·43	−0·51	−0·58	−0·52	−0·46	−0·21	−0·27	−0·19
18	−0·42	−0·39	−0·38	−0·38	−0·40	−0·39	−0·50	−0·86	−0·73	−0·64	−0·74	−0·61	−0·48	−0·52	−0·60	−0·53	−0·54	−0·28	−0·33	−0·25
18	−0·43	−0·40	−0·42	−0·37	−0·42	−0·35	−0·50	−0·90	−0·75	−0·40	−0·71	−0·63	−0·58	−0·62	−0·62	−0·66	−0·53	−0·19	−0·29	−0·24
19	−0·42	−0·39	−0·39	−0·42	−0·43	−0·39	−0·59	−0·96	−0·64	−0·56	−0·58	−0·59	−0·64	−0·68	−0·69	−0·83	−0·49	−0·25	−0·28	−0·19
20	−0·37	−0·36	−0·38	−0·38	−0·38	−0·33	−0·57	−0·87	−0·57	−0·52	−0·46	−0·53	−0·65	−0·65	−0·67	−0·66	−0·50	−0·20	−0·21	−0·16
20	−0·33	−0·36	−0·33	−0·32	−0·33	−0·10	−0·45	−0·87	−0·59	−0·47	−0·37	−0·50	−0·58	−0·60	−0·65	−0·63	−0·55	−0·16	−0·26	−0·18
21	−0·22	−0·25	−0·24	−0·32	−0·25	−0·21	−0·45	−0·72	−0·49	−0·33	−0·37	−0·46	−0·49	−0·58	−0·56	−0·71	−0·54	−0·14	−0·23	−0·10
22	−0·14	−0·15	−0·16	−0·24	−0·19	−0·23	−0·25	−0·55	−0·42	−0·21	−0·16	−0·33	−0·45	−0·59	−0·50	−0·51	−0·48	−0·08	−0·18	−0·03
22	−0·09	−0·03	−0·02	−0·07	−0·10	−0·05	−0·05	−0·50	−0·39	−0·24	−0·23	−0·29	−0·22	−0·34	−0·31	−0·18	−0·22	−0·19	−0·17	−0·05
23	+0·01	0·00	+0·02	−0·09	0·00	+0·05	0·00	−0·11	−0·37	−0·47	−0·36	−0·30	−0·11	−0·16	−0·24	−0·21	−0·17	−0·14	−0·15	−0·06
0	+0·11	+0·11	+0·07	−0·04	+0·14	+0·10	+0·22	+0·09	−0·30	−0·27	−0·32	−0·02	+0·16	+0·10	−0·02	−0·04	+0·03	−0·04	−0·09	−0·07
1	+0·21	+0·08	+0·20	+0·24	+0·25	+0·18	+0·57	+0·09	+0·01	+0·08	+0·03	+0·31	+0·35	+0·30	+0·16	+0·21	+0·24	−0·06	−0·06	−0·07
2	+0·26	+0·07	+0·14	+0·22	+0·21	+0·12	+0·54	+0·10	+0·08	+0·03	−0·03	+0·21	+0·27	+0·20	+0·20	+0·21	+0·24	+0·04	+0·02	−0·04
3	+0·27	+0·14	+0·19	+0·22	+0·21	+0·07	+0·43	+0·29	+0·33	+0·25	+0·23	+0·31	+0·20	+0·18	+0·15	+0·19	+0·21	+0·29	+0·12	+0·11
4	+0·20	+0·09	+0·28	+0·34	+0·26	+0·17	+0·61	+0·69	+0·57	+0·56	+0·63	+0·58	+0·56	+0·52	+0·42	+0·55	+0·49	+0·28	+0·13	+0·20
5	+0·14	+0·11	+0·20	+0·29	+0·17	+0·14	+0·44	+0·72	+0·51	+0·44	+0·32	+0·45	+0·53	+0·53	+0·54	+0·52	+0·47	+0·15	+0·22	+0·17
6	+0·16	+0·19	+0·29	+0·36	+0·20	+0·21	+0·54	+0·68	+0·81	+0·70	+0·64	+0·65	+0·65	+0·62	+0·66	+0·49	+0·51	+0·28	+0·24	+0·17
6	+0·33	+0·38	+0·44	+0·49	+0·45	+0·39	+0·69	+0·96	+0·83	+0·74	+0·89	+0·72	+0·97	+0·94	+0·85	+0·65	+0·69	+0·29	+0·34	+0·23
7	+0·31	+0·33	+0·35	+0·36	+0·34	+0·30	+0·48	+0·90	+0·51	+0·41	+0·58	+0·44	+0·81	+0·80	+0·59	+0·53	+0·54	+0·18	+0·29	+0·22
8	+0·16	+0·16	+0·22	+0·19	+0·11	+0·10	+0·27	+0·80	+0·50	+0·41	+0·44	+0·31	+0·87	+0·77	+0·56	+0·52	+0·61	+0·16	+0·16	+0·09
9	+0·09	+0·10	+0·10	+0·08	−0·01	+0·07	+0·11	+0·72	+0·42	+0·19	+0·15	+0·14	+0·84	+0·74	+0·71	+0·56	+0·53	+0·01	+0·12	+0·03
10	−0·03	−0·02	+0·02	−0·09	−0·11	−0·01	+0·08	+0·47	+0·15	+0·07	−0·06	+0·14	+0·68	+0·57	+0·57	+0·38	+0·55	−0·03	−0·03	−0·06
11	−0·13	−0·12	−0·14	−0·24	−0·21	−0·15	−0·06	+0·15	−0·01	−0·08	−0·21	+0·19	+0·33	+0·27	+0·30	+0·20	+0·22	−0·06	−0·05	−0·06
12	−0·31	−0·23	−0·26	−0·36	−0·28	−0·25	−0·28	−0·09	−0·09	−0·03	−0·19	+0·11	+0·12	+0·03	+0·03	+0·01	+0·04	−0·09	−0·07	−0·09
13	−0·32	−0·20	−0·33	−0·43	−0·35	−0·33	−0·67	−0·46	−0·27	−0·25	−0·31	−0·17	−0·09	−0·15	−0·25	−0·14	−0·14	−0·19	−0·11	−0·10
14	−0·27	−0·01	−0·31	−0·33	−0·32	−0·32	−0·44	−0·53	−0·39	−0·37	−0·48	−0·31	−0·22	−0·25	−0·39	−0·26	−0·22	−0·16	−0·18	−0·14
15	−0·33	−0·04	−0·35	−0·40	−0·39	−0·38	−0·43	−0·69	−0·61	−0·55	−0·63	−0·42	−0·39	−0·41	−0·50	−0·41	−0·36	−0·06	−0·20	−0·23
16	−0·29	−0·14	−0·25	−0·34	−0·26	−0·24	−0·44	−0·84	−0·66	−0·59	−0·55	−0·54	−0·34	−0·29	−0·32	−0·34	−0·26	−0·19	−0·17	−0·15
17	−0·28	−0·24	−0·28	−0·41	−0·29	−0·29	−0·38	−0·96	−0·84	−0·74	−0·57	−0·85	−0·62	−0·54	−0·64	−0·66	−0·54	−0·21	−0·22	−0·17
18	−0·32	−0·23	−0·37	−0·46	−0·38	−0·35	−0·62	−1·03	−0·87	−0·72	−0·75	−0·77	−0·75	−0·69	−0·71	−0·79	−0·66	−0·23	−0·28	−0·19
19	−0·37	−0·31	−0·40	−0·45	−0·38	−0·43	−0·54	−0·88	−0·70	−0·59	−0·55	−0·57	−0·57	−0·63	−0·58	−0·66	−0·59	−0·23	−0·29	−0·21
19	−0·31	−0·20	−0·35	−0·40	−0·35	−0·40	−0·50	−0·80	−0·60	−0·55	−0·50	−0·50	−0·50	−0·50	−0·50	−0·50	−0·45	−0·16	−0·20	−0·15

Diurnal Tide in Height at Low Water. First Division.

imate time illuaha. 1842.		Kilbaha.	Kilrush.	Fornes Island.	Limerick.	Casleh Bay.	Galway.	Old Head.	Mullaghmore.	Buncrana.	Port Rush.	Carrowkeel.	Ballycastle.	Glenarm.	Donaghadee.	Ardglass.	Clogher Head.	Kingstown.	Dunmore East.	New Ross.	Passage West.
	hrs.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
22.	23	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	+0·40	.....	.....	.....	.....	.....
23.	23	.....	.....	.....	.....	.....	.....	+0·47	+0·30	−0·30	−0·01	−0·32	.....	+0·65	+0·63	+0·56	+0·54	+0·59	−0·18	+0·03	.....
25.	0	.....	+0·64	.....	.....	+0·71	+0·60	+0·64	+0·14	−0·21	−0·06	−0·39	.....	+0·73	+0·72	+0·55	+0·59	+0·58	−0·15	+0·03	−0·12
26.	1	+0·52	+0·50	.....	+1·13	+0·57	+0·58	+0·53	+0·15	−0·19	−0·36	−0·33	.....	+0·43	+0·42	+0·35	+0·36	+0·32	−0·31	−0·19	−0·12
27.	1	+0·46	+0·44	+0·55	+0·63	+0·46	+0·49	+0·36	+0·05	−0·39	−0·41	−0·26	.....	+0·36	+0·39	+0·38	+0·41	+0·25	−0·26	−0·10	−0·08
28.	2	+0·49	+0·57	+0·65	+0·75	+0·55	+0·57	+0·41	+0·09	−0·33	−0·13	−0·09	+0·14	+0·40	+0·46	+0·48	+0·42	+0·04	−0·17	−0·06	−0·08
29.	3	+0·44	+0·50	+0·51	+0·61	+0·50	+0·52	+0·33	+0·16	−0·26	−0·02	−0·10	+0·14	+0·15	+0·32	+0·33	+0·37	+0·31	−0·23	−0·17	−0·04
30.	4	+0·39	+0·40	+0·51	+0·63	+0·41	+0·52	+0·36	+0·26	−0·11	+0·06	+0·03	+0·14	+0·19	+0·30	+0·30	+0·32	+0·32	−0·11	−0·11	−0·01
1.	5	+0·39	+0·41	+0·51	+0·95	+0·42	+0·45	+0·60	+0·50	+0·07	+0·14	+0·21	+0·09	+0·25	+0·34	+0·31	+0·38	+0·35	−0·11	−0·10	−0·10
2.	6	+0·22	+0·19	+0·32	+0·47	+0·18	+0·26	+0·34	+0·41	−0·05	−0·06	0·00	−0·11	+0·19	+0·21	+0·18	+0·18	+0·22	−0·13	−0·08	−0·07
3.	7	+0·24	+0·16	+0·23	+0·29	+0·13	+0·13	+0·25	+0·28	+0·06	+0·03	+0·04	+0·05	+0·27	+0·32	+0·28	+0·18	+0·29	+0·01	+0·07	+0·03
4.	8	+0·20	+0·18	+0·10	−0·12	+0·16	+0·11	+0·28	+0·08	+0·03	+0·02	+0·06	+0·15	+0·43	+0·35	+0·29	−0·04	+0·16	+0·11	+0·25	+0·11
5.	9	−0·14	−0·05	−0·21	−0·55	−0·19	−0·28	−0·04	+0·17	−0·17	−0·09	+0·01	−0·04	+0·10	+0·01	−0·11	−0·17	−0·17	+0·04	0·00	0·00
6.	10	−0·34	−0·40	−0·49	−0·72	−0·45	−0·48	−0·26	+0·35	+0·05	−0·05	−0·01	−0·04	−0·17	−0·16	−0·16	−0·09	−0·18	0·00	−0·05	−0·04
7.	11	−0·40	−0·60	−0·74	−1·16	−0·52	−0·61	−0·27	+0·40	+0·11	+0·11	+0·03	+0·06	−0·34	−0·39	−0·20	−0·37	−0·42	+0·05	−0·17	+0·01
8.	12	−0·16	−0·71	−0·90	−1·37	−0·65	−0·68	−0·56	+0·09	−0·03	+0·07	−0·06	+0·02	−0·62	−0·64	−0·64	−0·64	−0·64	+0·06	−0·19	0·00
9.	12	−0·34	−0·42	−0·61	−0·57	−0·46	−0·56	−0·62	+0·02	+0·26	+0·47	+0·20	+0·37	−0·39	−0·40	−0·38	−0·38	−0·32	+0·08	+0·03	+0·07
10.	13	−0·23	−0·08	−0·51	−0·61	−0·46	−0·57	−0·73	−0·05	+0·12	+0·22	+0·16	+0·14	−0·51	−0·44	−0·34	−0·34	−0·27	+0·05	+0·23	+0·38
11.	14	−0·60	−0·47	−0·58	−0·79	−0·67	−0·68	−0·82	−0·46	+0·09	+0·24	+0·17	+0·15	−0·70	−0·63	−0·58	−0·66	−0·56	+0·09	+0·06	+0·12
12.	14	−0·44	−0·29	−0·55	−0·53	−0·50	−0·63	−0·61	−0·43	−0·09	+0·33	+0·35	+0·07	−0·57	−0·50	−0·49	−0·49	−0·42	+0·10	+0·19	+0·16
13.	15	−0·39	−0·41	−0·55	−0·50	−0·39	−0·48	−0·55	−0·36	+0·10	+0·11	+0·25	−0·21	−0·64	−0·56	−0·58	−0·61	−0·56	+0·19	−0·10	+0·09
14.	16	−0·27	−0·22	−0·29	−0·27	−0·27	−0·29	−0·44	−0·44	+0·09	+0·05	+0·13	−0·28	−0·39	−0·38	−0·44	−0·47	−0·44	+0·15	+0·09	+0·07
15.	17	−0·20	−0·30	−0·16	−0·14	−0·24	−0·20	−0·37	−0·44	0·00	−0·01	+0·04	−0·30	−0·28	−0·23	−0·26	−0·14	−0·31	+0·16	+0·10	+0·11
16.	18	−0·07	−0·09	−0·05	+0·05	−0·16	−0·39	−0·39	−0·45	+0·01	−0·10	+0·04	−0·27	−0·24	−0·19	+0·14	−0·23	−0·21	+0·06	−0·07	−0·04
17.	20	+0·13	+0·12	+0·14	+0·14	+0·11	+0·03	−0·12	−0·20	0·00	−0·06	+0·01	−0·03	−0·01	+0·02	+0·08	−0·03	0·00	+0·01	−0·02	−0·08
18.	21	+0·26	+0·28	+0·27	+0·26	+0·28	+0·25	+0·18	−0·19	−0·09	−0·10	+0·03	−0·08	−0·11	+0·17	+0·10	+0·11	+0·11	−0·09	−0·07	−0·02
19.	22	+0·33	+0·34	+0·36	+0·39	+0·36	+0·41	+0·33	−0·25	−0·15	−0·13	−0·04	−0·13	+0·09	+0·31	+0·23	+0·26	+0·24	−0·06	−0·14	−0·05
20.	22	+0·41	+0·39	+0·45	+0·40	+0·45	+0·46	+0·52	−0·07	−0·14	−0·18	−0·12	−0·14	+0·42	+0·44	+0·36	+0·25	+0·32	−0·09	−0·15	−0·07
21.	23	+0·43	+0·39	+0·46	+0·32	+0·51	+0·44	+0·56	+0·01	−0·16	−0·25	−0·21	−0·19	+0·45	+0·46	+0·36	+0·31	+0·29	−0·10	−0·11	−0·05
23.	0	+0·39	+0·35	+0·36	+0·27	+0·42	+0·33	+0·48	0·00	−0·21	−0·34	−0·24	−0·26	+0·47	+0·46	+0·38	+0·25	+0·27	−0·18	−0·18	−0·11
24.	0	+0·41	+0·23	+0·40	+0·31	+0·45	+0·41	+0·48	−0·01	−0·28	−0·40	−0·32	−0·31	+0·44	+0·44	+0·35	+0·21	+0·31	−0·20	−0·13	−0·11
25.	1	+0·36	+0·25	+0·46	+0·32	+0·46	+0·46	+0·47	+0·05	−0·31	−0·41	−0·34	−0·22	+0·38	+0·38	+0·30	+0·31	+0·27	−0·24	−0·15	−0·07
26.	1	+0·33	+0·30	+0·41	+0·26	+0·45	+0·41	+0·40	+0·06	−0·24	−0·33	−0·25	−0·13	+0·31	+0·32	+0·30	+0·32	+0·27	−0·15	−0·15	−0·07
27.	2	+0·35	+0·29	+0·38	+0·35	+0·42	+0·47	+0·34	−0·03	−0·19	−0·28	−0·25	−0·09	+0·23	+0·26	+0·28	+0·30	+0·26	−0·15	−0·14	−0·11
28.	2	+0·31	+0·36	+0·37	+0·55	+0·41	+0·52	+0·39	+0·04	−0·14	−0·23	−0·23	−0·04	+0·26	+0·28	+0·31	+0·35	+0·25	−0·28	−0·17	−0·10
29.	3	+0·28	+0·31	+0·30	+0·39	+0·28	+0·41	+0·35	−0·05	−0·19	−0·18	−0·12	+0·02	+0·19	+0·19	+0·26	+0·28	+0·27	−0·15	−0·18	−0·10
30.	4	+0·27	+0·21	+0·32	+0·48	+0·23	+0·29	+0·39	+0·21	+0·04	+0·04	+0·11	+0·11	+0·20	+0·19	+0·27	+0·31	+0·27	−0·06	−0·05	−0·01
31.	5	+0·19	+0·15	+0·28	+0·41	+0·18	+0·17	+0·43	+0·34	+0·15	+0·07	+0·22	0·00	+0·09	+0·18	+0·25	+0·27	+0·19	−0·04	+0·01	0·00
t 1.	6	−0·02	+0·07	+0·09	+0·10	+0·01	0·00	+0·21	+0·32	+0·11	+0·01	+0·01	−0·11	+0·14	+0·17	+0·11	+0·12	+0·11	+0·06	+0·05	0·00
2.	8	−0·15	−0·08	−0·09	−0·21	−0·14	−0·08	+0·01	+0·27	+0·16	+0·06	+0·02	+0·02	+0·25	+0·16	+0·10	+0·23	+0·06	+0·09	+0·09	0·00
3.	9	−0·31	−0·29	−0·30	−0·48	−0·22	−0·16	−0·12	+0·14	+0·03	−0·07	−0·04	0·00	+0·12	+0·02	−0·12	+0·26	−0·21	+0·01	−0·04	−0·03
4.	9	−0·37	−0·39	−0·44	−0·75	−0·33	−0·02	−0·36	−0·01	−0·01	−0·09	+0·12	−0·10	−0·26	−0·35	−0·40	−0·36	−0·39	−0·03	−0·09	−0·02
5.	10	−0·51	−0·45	−0·60	−0·70	−0·58	0·00	−0·37	−0·04	+0·11	+0·19	+0·26	+0·06	−0·55	−0·58	−0·50	−0·42	−0·43	+0·11	−0·04	+0·08
6.	11	−0·45	−0·32	−0·57	−0·47	−0·49	−0·42	−0·49	+0·18	0·00	+0·08	+0·11	+0·08	−0·71	−0·66	−0·62	−0·69	−0·50	+0·08	−0·01	+0·08
7.	12	−0·71	−0·52	−0·68	−0·35	−0·60	−0·64	−0·72	−0·23	−0·03	+0·27	+0·20	+0·15	−1·04	−0·88	−0·82	−0·64	−0·64	+0·06	−0·09	+0·06
8.	12	−0·48	−0·51	−0·49	−0·11	−0·61	−0·42	−0·61	−0·17	+0·34	+0·55	+0·47	+0·50	−0·66	−0·58	−0·55	−0·40	−0·39	+0·04	+0·13	+0·02
9.	13	−0·34	−0·34	−0·34	−0·16	−0·40	−0·41	−0·63	−0·03	+0·26	+0·44	+0·29	+0·26	−0·53	−0·46	−0·45	−0·41	−0·41	+0·08	+0·12	+0·07
10.	14	−0·28	−0·30	−0·28	−0·12	−0·29	−0·26	−0·57	+0·02	+0·33	+0·26	+0·31	+0·13	−0·42	−0·34	−0·36	−0·36	−0·38	+0·11	+0·08	+0·07
11.	14	−0·15	−0·24	−0·18	−0·18	−0·15	−0·08	−0·57	−0·10	+0·37	+0·40	+0·36	+0·07	−0·23	−0·19	−0·19	−0·17	−0·17	+0·24	+0·10	+0·09
12.	15	−0·04	−0·14	−0·09	−0·19	−0·05	−0·20	−0·51	−0·09	+0·27	+0·27	+0·16	−0·03	−0·09	−0·07	−0·09	−0·13	−0·14	+0·20	+0·04	+0·06
13.	16	+0·05	−0·08	+0·04	+0·05	+0·06	+0·10	−0·27	−0·07	+0·09	+0·03	+0·01	−0·03	−0·02	+0·03	−0·01	−0·03	−0·06	+0·10	+0·04	+0·04
14.	18	+0·17	+0·18	+0·26	+0·40	+0·21	+0·25	+0·04	−0·04	+0·08	−0·02	−0·01	+0·08	+0·18	+0·24	+0·21	+0·10	+0·13	+0·01	−0·04	−0·02
15.	19	+0·23	+0·28	+0·27	+0·30	+0·27	+0·29	+0·20	−0·04	−0·07	−0·09	−0·13	+0·07	+0·20	+0·26	+0·21	+0·10	+0·20	−0·05	−0·09	−0·05
16.	20	+0·34	+0·37	+0·35	+0·38	+0·41	+0·45	+0·39	+0·02	−0·11	−0·10	−0·14	+0·16	+0·39	+0·41	+0·32	+0·21	+0·21	0·00	−0·05	−0·01
17.	21	+0·35	+0·37	+0·40	+0·46	+0·40	+0·38	+0·39	−0·02	−0·14	−0·16	−0·20	+0·09	+0·52	+0·50	+0·38	+0·31	+0·19	−0·16	−0·13	−0·06
18.	22	+0·39	+0·42	+0·49	+0·56	+0·43	+0·36	+0·50	−0·11	−0·09	−0·10	−0·10	+0·07	+0·59	+0·54	+0·39	+0·40	+0·32	−0·08	+0·02	−0·02
19.	22	+0·38	+0·43	+0·48	+0·46	+0·42	+0·47	+0·65	−0·05	−0·12	−0·32	−0·22	−0·01	+0·59	+0·66	+0·54	+0·54	+0·49	−0·15	+0·02	−0·07
20.	23	+0·42	+0·34	+0·43	+0·28	+0·42	+0·47	+0·38	−0·07	−0·33	−0·45	−0·43	−0·26	+0·39	+0·45	+0·34	+0·33	+0·32	−0·25	−0·16	−0·10
21.	23	+0·36	+0·27	+0·37	+0·30	+0·35	+0·36	+0·18	−0·06	−0·33	−0·43	−0·46	−0·41	+0·30	+0·28	+0·21	+0·17	+0·11	−0·28	−0·18	−0·05
23.	0	+0·32	+0·25	+0·35	+0·25	+0·33	+0·40	+0·25	−0·07	−0·33	−0·43	−0·45	−0·35	+0·40	+0·35	+0·26	+0·25	+0·20	−0·28	−0·15	−0·07

Times of Evanescence of Diurnal Tide in Height.

		d.	d.	d.	d.
Kilbaha	High water.	July 1·73	July 14·97	July 30·72	Aug. 12·03
	Low water.	4·59	17·01	31·84	13·15
Kilrush	High water.	(1·8)	15·17	31·46	12·27
	Low water.	4 30	17·03	32·29	13·68
Foynes Island	High water.	1·98	14·74	30·74	12·31
	Low water.	4·27	16·98	32·64	13·19
Limerick	High water.	(1·9)	(14·7)	31·30	11·86
	Low water.	4·00	16·86	32·54	13·05
Casleh Bay	High water.	2·34	13·98	30·82	11·55
	Low water.	4·10	17·21	32·41	13·10
Galway	High water.	3·83.	13·71	30·77	11·99
	Low water.	4·24	17·50	32·56	13·04
Old Head	High water.	(4·8)	15·62	31·36	12·38
	Low water.	5·22	18·11	33·46	14·80
Mullaghmore	High water.	4·61	17·63	32·53	14·17
	Low water.	(7·0)	(20·0)	(35·0)	(16·0)
Buncrana	High water.	5·38	17·56	33·32	13·74
	Low water.	(7·4)	(20·0)	(35·0)	15·63
Port Rush	High water.	5·00	17·26	33·43	13·47
	Low water.	(7·4)	(20·0)	(35·0)	14·92
Carrowkeel	High water.	4·96	16·66	33 26	12·69
	Low water.	(7·4)	(20·0)	(35·0)	(14·9)
Ballycastle	High water.	3·80	17·68	32·37	14·51
	Low water.	(6·4)	(19·0)	(34·0)	(13 9)
Glenarm	High water.	2·80	18·11	31·72	15·62
	Low water.	5·92	18·78	34·24	13·84
Donaghadee	High water.	2·82	17·91	31·85	15·35
	Low water.	5·34	17·92	33·18	13·56
Ardglass	High water.	3·76	18·00	32·82	15·10
	Low water.	5·57	16·11	33·47	13·71
Clogher Head	High water.	3·51	17·46	32·48	15·05
	Low water.	4·82	18·23	34·34	14·60
Kingstown	High water.	3·70	17·62	32·22	15·44
	Low water.	5·10	18·41	33·25	14·16
Dunmore East	High water.	6·01	17·44	33·40	12·39
	Low water.	3·56	18·31	32·13	15·88
New Ross	High water.	(6·0)	(17·4)	33·57	12·95
	Low water.	(3·5)	(18·3)	(32 1)	(15·8)
Passage West	High water.	(5·0)	(16·4)	33·76	12·27
	Low water.	(2·5)	16·91	(31·1)	14·75
Castle Townsend	High water.	(3·4)	(15·7)	(32·24)	(12·15)
	Low water.	(3·5)	(17·0)	(31·5)	(13·95)

The numbers inclosed in brackets are supplied by conjecture, where the irregularity of the tides made it difficult to discover with accuracy the times of evanescence. A small error in these will produce very little error in the result for maximum coefficient. The numbers at Castle Townsend (where the diurnal tide is very small) are the means between those for the adjacent stations, Passage West and Kilbaha. Courtown is omitted, as the peculiarity of the tides there made it impossible to take diurnal tides at high water and low water in the same manner as for the other stations.

The times when the moon's declination vanished are June $28^{d}\cdot89$, July $12^{d}\cdot14$, July $26^{d}\cdot18$, and August $8^{d}\cdot50$. In stating these, however, I must warn the reader that these are not the only elements on which the time of evanescence of diurnal tide depends, as will appear in the next section.

Attempts were made to determine, in the same manner, the times of evanescence of diurnal tide in time. The irregularities were however so great that in most cases it was useless to attempt to assign the day: in the following instances only did the results appear at all trustworthy.

Times of Evanescence of Diurnal Tide in Time.

		d	d	d	d
Mullaghmore	Low water.	July 7·19	July 20·76	July 31·96	Aug. 14·27
Buncrana	Low water.	3·30	20·41	30·77	16·02
Port Rush	Low water.	4·69	21·32	31·32	16·58
Carrowkeel	Low water.	1·37	19·01	30·50	16·39
Ballycastle	Low water.	5·11	18·16	36·43	15·05
Donaghadee	High water.	4·11	19·07	32·59	16·18
	Low water.	6·13	14·18	33·94	13·15

It will be remembered that the time of evanescence of diurnal tide in time at low water ought to coincide with that for height at high water, and *vice versâ*. The comparison of this table with that for height is not very satisfactory.

The maximum values of diurnal tide were deduced in all cases by the process explained a short time since, adopting for the times of evanescence the days given in the first table (or that from heights), and using the high water evanescence in height with the low water diurnal tide in time, and *vice versâ*. The following are the results:—

Maximum Values of Diurnal Tide, First Division: deduced from heights.

	High water.			Low water.		
	July 1—14.	July 14—31.	July 31—Aug. 12.	July 4—17.	July 17—32.	Aug. 1—13.
	ft.	ft.	ft.	ft.	ft.	ft.
Kilbaha	+0·28	−0·49	+0·30	−0·47	+0·52	−0·50
Kilrush	+0·20	−0·38	+0·25	−0·52	+0·42	−0·47
Foynes Island	+0·30	−0·45	+0·33	−0·74	+0·52	−0·58
Limerick	+0·30	−0·49	+0·36	−0·96	+0·49	−0·53
Casleh Bay	+0·27	−0·41	+0·30	−0·64	+0·52	−0·55
Galway	+0·24	−0·42	+0·25	−0·77	+0·57	−0·35
Old Head	+0·46	−0·61	+0·64	−0·69	+0·58	−0·74
Mullaghmore	+0·83	−0·80	+0·85	−0·30	+0·14	−0·08
Buncrana	+0·64	−0·75	+0·66	+0·05	−0·16	+0·25
Port Rush	+0·50	−0·57	+0·60	+0·14	−0·28	+0·35
Carrowkeel	+0·44	−0·93	+0·60	+0·17	−0·19	+0·33
Ballycastle	+0·50	−0·66	+0·58	+0·05	−0·16	+0·16
Glenarm	+0·74	−0·66	+0·82	−0·60	+0·42	−0·55
Donaghadee	+0·64	−0·74	+0·74	−0·64	+0·41	−0·71
Ardglass	+0·55	−0·74	+0·69	−0·60	+0·38	−0·58
Clogher Head	+0·64	−0·69	+0·60	−0·55	+0·36	−0·57
Kingstown	+0·53	−0·57	+0·64	−0·55	+0·36	−0·52
Dunmore East	+0·17	−0·24	+0·25	+0·13	−0·22	+0·11
New Ross	+0·22	−0·33	+0·25	+0·05	−0·22	+0·05
Passage West	+0·08	−0·20	+0·17	+0·13	−0·11	+0·06
Castle Townsend	−0·02	−0·09	+0·05	−0·16	−0·01	−0·08

These results are on the whole satisfactory. There are, however, some general differences of magnitude among the different columns, which I am not able at present entirely to explain. I may remark that the moon was in perigee on July 9 and August 7, and in apogee on July 25.

Maximum Coefficient of Diurnal Tide in Time, First Division.

	Low water.			High water.		
	July 1—14.	July 14—31.	July 31—Aug. 12.	July 4—17.	July 17—32.	Aug. 1—13.
	m	m	m	m	m	m
Kilbaha	+ 5·31	− 2·69	+ 4·25	− 6·22	+ 3·70	−0·98
Kilrush	+ 1·34	− 1·87	+ 1·75	− 5·92	+ 3·70	−3·18
Foynes Island	+ 4·10	− 4·00	+ 5·11	− 5·63	+ 1·70	−3·84
Limerick	+12·23	− 8·86	+ 8·15	− 3·91	+ 0·69	−5·63
Casleh Bay	+ 2·51	− 2·40	+ 3·18	− 8·73	+ 6·06	−4·29
Galway	+ 5·03	− 6·97	+ 3·08	− 8·73	+ 3·64	−4·98
Old Head	+ 2·87	− 2·96	+ 0·66	− 4·32	+ 4·13	+0·26
Mullaghmore	+ 3·93	− 7·77	+10·56	−10·48	+ 2·06	−5·09
Buncrana	+10·90	− 1·51	+ 7·90	− 0·42	+ 0·34	−1·73
Port Rush	+21·43	− 4·85	+16·77	− 1·88	+ 7·28	−3·14
Carrowkeel	+17·93	− 3·35	+14·92	− 5·60	+ 2·71	−0·68
Ballycastle	+28·28	−31·00	+17·26	− 3·42	+18·05	−1·96
Glenarm	− 1·76	− 7·82	+ 4·09	+ 0·66	+ 2·68	−2·20
Donaghadee	+ 0·04	− 6·58	+ 0·57	− 5·27	+ 1·72	−4·52
Ardglass	+ 1·72	− 4·41	+ 2·70	+ 0·07	− 4·47	−0·35
Clogher Head	+ 3·80	− 5·10	+ 1·86	− 0·01	− 2·47	−3·10
Kingstown	+ 4·32	− 5·46	+ 5·65	− 4·35	+ 0·52	−3·92
Dunmore East	+ 0·54	− 2·77	+ 3·31	+ 3·86	− 0·24	−2·40
New Ross	− 1·76	− 0·48	+ 3·45	+ 2·54	− 2·05	−4·99
Passage West	+ 2·71	+ 2·68	+ 0·96	+ 0·23	+ 0·70	−0·51
Castle Townsend	− 6·57	+ 2·12	− 1·26	− 2·49	+ 1·37	−3·67

After the statement which I have given (in the second section) of the difficulty of fixing upon times of high and low water, it will not be surprising that considerable irregularities exist among these numbers. Their agreement nevertheless is sufficient to show that the diurnal tide in time of low water is great at Limerick, and very great at all the stations from Buncrana to Ballycastle. At Mullaghmore and Ballycastle it is also great at high water. The increase in numbers at low water from Kilrush to Foynes Island and Limerick, would seem to show that diurnal tide in time at low water increases considerably in ascending a river. It would appear, however (as seems, *à priori*, probable), that this holds only when there is at the same time a considerable diurnal tide in height, of such a nature that a depression of height accompanies a retardation of time. This is supported entirely by the analogy of the course of low water at ordinary semidiurnal tide: where, as will appear in this paper, and as is known from other observations, and as also appears from theory*, the progress of the phase of low water up a river is slower as the water is shallower at low water. At New Ross, considered with relation to Dunmore East, the diurnal tide in time of low water is not sensibly increased; and here there is no large diurnal tide in height. The large numbers in the neighbourhood of Ballycastle do not depend on this cause.

Maximum Coefficient of Diurnal Tide in Height, First Division, as inferred from Diurnal Tide in Time.

	High water (from times of low water).			Low water (from times of high water).		
	July 1—14.	July 14—31.	July 31—Aug. 12.	July 4—17.	July 17—32.	Aug. 1—13.
	ft.	ft.	ft.	ft.	ft.	ft.
Kilbaha	+0·46	−0·18	+0·37	−0·59	+0·28	−0·13
Kilrush	+0·11	−0·14	+0·12	−0·62	+0·30	−0·37
Foynes Island....	+0·41	−0·40	+0·61	−0·68	+0·19	−0·50
Limerick.....	+1·88	−1·16	+1·35	−0·61	+0·06	−0·81
Casleh Bay.....	+0·24	−0·16	+0·30	−0·92	+0·46	−0·45
Galway	+0·60	−0·51	+0·45	−0·87	+0·28	−0·68
Old Head	+0·28	−0·19	+0·07	−0·40	+0·24	+0·02
Mullaghmore	+0·16	−0·42	+1·02	−0·78	+0·17	−0·33
Buncrana	+0·97	−0·15	+0·75	−0·15	+0·04	−0·25
Port Rush	+0·77	−0·13	+0·73	−0·14	+0·14	−0·13
Carrowkeel.....	+0·97	−0·20	+0·85	−0·25	+0·11	−0·08
Ballycastle	+0·75	−0·42	+0·50	−0·11	+0·28	−0·08
Glenarm.....	−0·07	−0·31	+0·25	+0·05	+0·13	−0·13
Donaghadee	+0·06	−0·46	+0·10	−0·39	+0·11	−0·45
Ardglass.....	+0·27	−0·40	+0·33	−0·04	+0·37	−0·11
Clogher Head ..	+0·45	−0·45	+0·32	−0·03	−0·19	−0·41
Kingstown	+0·44	−0·40	+0·57	−0·39	+0·46	−0·38
Dunmore East ..	+0·04	−0·20	+0·34	+0·31	−0·02	−0·20
New Ross	−0·19	−0·06	+0·37	+0·26	−0·20	−0·43
Passage West...	+0·26	+0·17	+0·09	+0·02	+0·08	−0·04
Castle Townsend..	−0·45	+0·12	−0·15	−0·17	+0·09	−0·24

These numbers ought, upon the theoretical expressions for the tides given in an earlier part of this section, to agree with the numbers in the first table in page 19.

* Encycl. Metropol., *Tides and Waves*, Art. 208.

Upon comparing them it appears that there is a very good agreement of the numbers of the littoral stations, at both high and low waters, as far as Mullaghmore or even Buncrana; and, for high water only, as far as Ballycastle. There is then great discordance till we arrive nearly at Kingstown; in a short time after this the diurnal tide becomes so small that we are less surprised at apparent discordances. From the number of the instances in which the agreement is, upon the whole, pretty good, I form my opinion that the discordance between Buncrana and Kingstown is not accidental. I have little doubt that in this channel between Ireland and Scotland (which every accurate determination shows to be a critical part for the tides), the law of diurnal tide assumes a form differing much from that supposed in the investigation. It is, however, practically almost impossible to trace this law from observations.

The results for diurnal tide used in the subsequent investigations are those in the table of pages 15 and 16 deduced from observed heights only.

Section IV.—*Theory of Diurnal Tide as referred to the actions of the Sun and Moon.*

The present section will contain little more than the account of a series of failures of investigations. But the examination of these is usually so instructive that I think it desirable to state the heads of each of the unsuccessful attempts.

In order to explain the theoretical difficulties of this investigation, the following remarks may not perhaps be misplaced.

It is not possible to separate the effects of the sun and moon by comparison of a mass of observed diurnal tides near one solstice with a similar mass at the opposite solstice. For, although (in consequence of the opposite state of the moon's declination at a given phase of the lunation) the lunar diurnal tide is different in sign, yet the solar diurnal tide is also different in sign; and thus the two diurnal tides are mingled in the same degree at both solstices. The same applies if the observations are at any opposite seasons of the year.

It is possible to separate the two effects by comparing diurnal tides near a solstice with diurnal tides near an equinox; as, in the latter, the solar diurnal tide vanishes.

Generally, it is possible to separate them by comparing two masses of diurnal tides observed at intervals of three months; as for the high (or low) waters corresponding to a given right ascension and declination of the moon in the two masses, the sun will have widely different positions in hour-angle, and therefore its effects at those two instants will be widely different.

The proportion of the effects of the sun and the moon cannot be ascertained from a single series of observations, extending through a period so short that the sun's position may be considered invariable. This will be shown by showing that the two effects, of the sun and of the moon, in producing diurnal tide at high water, follow sensibly the same law, and when added together give a compound effect following the same law. Thus: the time of high water bears a nearly invariable relation to the

time of moon's transit; and therefore, at high water, the lunar diurnal tide is always in nearly the same phase, and has no variation except from the variation of its coefficient. The magnitude of the diurnal tide at semidiurnal high water may therefore be represented by coefficient $\times \sin\beta$; and that at semidiurnal low water by coefficient $\times \cos\beta$; where β is constant. This coefficient is proportional to the sine of the moon's declination at some time previous, or (nearly) proportional to the sine of the moon's right ascension for some time previous, or to the sine of the moon's hour-angle from the sun altered by a constant. For the solar diurnal tide, the coefficient is constant, but the phase varies every day. As the time of high water bears a nearly invariable relation to the time of moon's transit, the phase of solar diurnal tide at high water must depend upon the moon's hour-angle from the sun altered by a constant, and therefore the magnitude of solar diurnal tide will be proportional to the sine of the moon's hour-angle from the sun altered by a constant. Thus, putting $\leftmoon - \odot$ for the excess of the moon's right ascension above the sun's, the lunar diurnal tide at the time of high water will be represented by $a.\sin\beta.\sin\{\leftmoon - \odot + A\}$, and the solar diurnal tide at the same time will be represented by $b.\sin\{\leftmoon - \odot + B\}$; and these, when added together, give a result of the same form, $c.\sin\{\leftmoon - \odot + C\}$. And it is impossible to say whether this term, as given by observation, is entirely due to one or other of the two actions or to both combined; because we have no *à priori* means of saying what is the coefficient a or b of either of the separate terms; or what is the relation of the time of either high diurnal tide to the time of transit of the body which causes it, upon which A and B will depend. Everything here said with regard to semidiurnal high water applies also to semidiurnal low water; the only difference being that the angles β and B must be increased about 90° for semidiurnal low water.

The unknown quantities in the problem of diurnal tide are the following:—The interval anterior to the time of observation for which the moon's place is to be taken as governing the diurnal tide at the time of observation; the constant coefficient by which the sine of moon's declination for that anterior time is to be multiplied; the moon's hour-angle at the time of lunar diurnal high water; and the three similar quantities for the sun: in all, six unknown quantities. To determine these we have only the four following results of observation (or results equivalent to these four): the time of evanescence of diurnal tide at semidiurnal high water; the maximum of diurnal tide in high water, and the two similar quantities for low water. These are insufficient for the determination of the six unknown quantities; and we must try how we can reduce the latter number.

First, as the sun's declination is considered constant, the anterior interval for the sun's place is unimportant. And in fact, though the sun's declination during these observations (June 22 to August 25) was not invariable, yet an alteration of one day in the time for which its declination was taken as ruling the diurnal tide would not have been important. For the moon it would be very important.

Secondly, it seems probable that the moon's hour-angle at the time of lunar diurnal

high water does not differ much from the sun's hour-angle at the time of solar diurnal high water. The assumption of any constant difference, either $=0$ or having any assigned magnitude, reduces two of the unknown quantities to one.

The number of unknown quantities is thus made the same as the number of data, and the solution can therefore (speaking in a strictly algebraical sense) be effected, in general.

The following is the method by which the equations for the four unknown quantities may most conveniently be formed:—

From the ordinary facts of the tides, it seems probable that the coefficient of lunar diurnal tide may depend on the moon's declination at a few days, perhaps not exceeding five, anterior to the time of the tide. Let d_1 be the moon's declination at one day preceding the time of tide, d_3 the moon's declination at three days preceding the time of tide. Then we may express the coefficient of lunar diurnal tide by $p.\sin d_1+q.\sin d_3$; where by varying the proportions of p and q the coefficient may be made to depend on the moon's declination at any day near them; and by varying the magnitudes of p and q in the same proportion, the magnitude of the coefficient will be altered in that proportion.

The coefficient of solar diurnal tide may be represented with sufficient accuracy by $r.\sin D_1$, where D_1 is the sun's declination one day preceding the time of tide.

Let h be the solar hour of the tide. This is the same as the hour-angle of the sun to the west of the meridian. The phase of the solar diurnal tide will depend upon this angle diminished by some unknown constant s; and the elevation of the solar diurnal tide may be represented by $S.\sin D_1.\cos \overline{h-s}$.

Let t be the moon's time of transit. Then the moon's hour-angle west of the meridian is $h-t$. Therefore if the phase of lunar diurnal tide depended on the moon's hour-angle in the same manner in which the phase of solar diurnal tide depended on the sun's hour-angle, the elevation of the lunar diurnal tide would be represented by $(p.\sin d_1+q.\sin d_3)\cos \overline{h-t-s}$. But we know by the retardation of the period of spring tides, as well as by the theory of tidal waves affected by friction*, that in semidiurnal tides the lunar wave is more advanced in its phase with regard to the moon's hour-angle than the solar wave is with regard to the sun's hour-angle. We may conjecture, by analogy, that the same holds for diurnal tide. Putting α for this difference of advance of phase, the elevation of the lunar diurnal tide will be represented by $(p.\sin d_1+q.\sin d_3)\cos \overline{h-t-s+\alpha}$. And the compound effect of lunar and solar diurnal tides, expanding the cosines, will be

$$S.\sin D_1.(\cos h.\cos s+\sin h.\sin s)$$
$$+(p.\sin d_1+q.\sin d_3)(\cos \overline{h-t+\alpha}.\cos s+\sin \overline{h-t+\alpha}.\sin s).$$

Let $S.\cos s=w$, $S.\sin s=x$, $\frac{p}{S}=y$, $\frac{q}{S}=z$; and the expression becomes

* Encyclopædia Metropolitana, *Tides and Waves*, Art. 326.

$$\sin D_1.\cos h.w+\sin D_1.\sin h.x+\sin d_1.\cos \overline{h-t+\alpha}.wy+\sin d_1.\sin \overline{h-t+\alpha}.xy$$
$$+\sin d_3.\cos \overline{h-t+\alpha}.wz+\sin d_3.\sin \overline{h-t+\alpha}.xz.$$

It is to be remarked that, when w, x, y, and z, are ascertained (with an assumed value of α), the following more intelligible results will be extracted from them:—

$S=\sqrt{w^2+x^2}=$ solar coefficient of the sine of the sun's declination, for solar diurnal tide.

$s=$ the angle determined by the equation $\tan s=\frac{x}{w}$; it is the constant angle which is to be subtracted from the sun's hour-angle west at the time of observation, in order to give the angle on whose cosine depends the height of solar diurnal tide at the instant of observation.

Then, the lunar diurnal tide

$=(p.\sin d_1+q.\sin d_3)\cos \overline{h-t-s+\alpha}=S.(y.\sin d_1+z.\sin d_3).\cos \overline{h-t-s+\alpha}$;

and, {putting l for the moon's longitude measured from the intersection of its orbit with the equator, I for the sine of its inclination, and δ for the mean daily increase of longitude from transit to transit $=13^\circ\ 38'$}, $y.\sin d_1+z.\sin d_3=I(y.\sin l_1+z.\sin l_3)$

$=I\left(\frac{z+y}{2}.\overline{\sin l_3+\sin l_1}+\frac{z-y}{2}.\overline{\sin l_3-\sin l_1}\right)=I(\overline{z+y}.\cos\delta.\sin l_2+\overline{z-y}.\sin\delta.\cos l_2)$; or,

if $\tan\eta=\frac{z-y}{z+y}\tan\delta$, this quantity becomes $=I.\overline{z+y}.\cos\delta.\sec\eta.\sin\overline{l_2+\eta}$; or, making $\eta=n.\delta$, it becomes $=I.\overline{z+y}.\cos\delta.\sec\eta.\sin l_{2+n}=\overline{z+y}.\cos\delta.\sec\eta.\sin d_{2+n}$: and therefore the lunar diurnal tide $=S.\overline{z+y}.\cos\delta.\sec\eta.\sin d_{2+n}.\cos\overline{h-t-s+\alpha}$.

$\frac{M}{S}=\frac{\text{Effect of moon for given declination}}{\text{Effect of sun for same declination}}=\overline{z+y}.\cos\delta.\sec\eta.$

$M=S.\overline{z+y}.\cos\delta.\sec\eta=$ lunar coefficient of the sine of the moon's declination on a certain anterior day, for lunar diurnal tide.

$2+n=$ the time, in lunar days, earlier than the moon's Greenwich transit next preceding, for which the moon's declination is to be taken as governing the diurnal tide. This is correct for the time of high water, first division, and requires an alteration for other times. n is $=\frac{\eta}{13^\circ\ 38'}$; and $\tan\eta=\frac{z-y}{z+y}\cdot\tan 13^\circ\ 38'$.

$s-\alpha=$ the constant angle which is to be subtracted from the moon's hour-angle, in the same manner as s from the sun's hour-angle.

The factors of the unknown terms w, x, wy, wz, xy, and xz, in the algebraical expression for the elevation produced by diurnal tide, were computed for high water and low water, first division, at Kilbaha, for every day throughout the observations. These computations would apply equally to the other stations, it being understood that certain constants (which the reader will easily investigate) depending on the longitude of the station and the time occupied by the passage of semidiurnal tide, are to be applied to the angles α and s. The hour-angles used for the moon were found by comparing the moon's time of transit at Greenwich with the time of Kil-

baha tide. The declinations were those for the times of transit at Greenwich one day and three days previous to the transit next preceding the tide in question. For the low waters of the first division, which follow the high waters of the first division by $\frac{1}{4}$th of a tidal day, the right ascensions and declinations ought to be taken for transit over the 6^h meridian; this was done most conveniently by correcting the coefficients when combined in groups, by the following formulæ. If the computed terms containing w and x for the moon are $W.w+X.x$ {W and X containing y and z}, then the corrected terms as altered for the change of right ascension, are

$$\left(W\cos\tfrac{\delta}{4}+X\sin\tfrac{\delta}{4}\right)w+\left(X\cos\tfrac{\delta}{4}-W\sin\tfrac{\delta}{4}\right)x.$$

And if the computed terms containing y and z are $Y.y+Z.z$ (Y and Z containing w and x), then the corrected terms as altered for the change of declination, are

$$\left(Y.\frac{\sin\left(2\delta+\frac{\delta}{4}\right)}{\sin 2\delta}-Z\frac{\sin\frac{\delta}{4}}{\sin 2\delta}\right)y+\left(Z\frac{\sin\left(2\delta-\frac{\delta}{4}\right)}{\sin 2\delta}+Y\frac{\sin\frac{\delta}{4}}{\sin 2\delta}\right)z.$$

No notice was taken of the changes of parallax; nor were the hour-angles referred to the moon's place one or three days previous (as in strictness of theory they ought); but as the observations extend over two whole lunations, it was supposed that the effects of these omissions would nearly disappear.

The factors of the unknown quantities were computed on the supposition that $\alpha=0$, and also on two other suppositions. It is easily seen, however, that the factors for any assumed value of α can be readily formed from those which hold for $\alpha=0$; and this computation is made most conveniently for the groups.

The numbers for high water were divided into groups related to the changes of sign of the factors of w and x. These groups were then combined in the order 1st+2nd−3rd−4th+5th+6th− &c. to form one equation, and in the order 1st−2nd−3rd+4th+5th− &c. to form another equation. The numbers for low water were treated in the same way. In subsequent operations, the groups were formed and combined in different orders.

But, in whatever way the groups were formed, they were so combined as to form four equations, each of which has the following form:

$$A.w+B.x+C.wy+D.wz+E.xy+F.xz=G.$$

To solve a system of four such equations is evidently no easy matter. Two methods of solution were principally relied on.

The first (and easiest) was, to make trial-substitutions to a great extent. The numbers −2, −1, 0, +1, +2, were substituted for w; the same numbers were substituted for x; the same numbers were also substituted for y and for z; and every possible combination of these numbers was used; making 625 trial-substitutions in each of the four equations. And when there seemed a probability of success, the substitutions for one or two of the numbers were greatly extended. Calling the re-

sults of one substitution in the four equations g, g', g'', g''' (the numbers resulting from the tidal observations being G, G′, G″, G‴), it was then necessary that

$$\frac{g'}{g}=\frac{G'}{G},\ \frac{g''}{g}=\frac{G''}{G},\ \frac{g'''}{g}=\frac{G'''}{G}.$$

By search among the quotients of the substitution, numbers were found approaching as near as possible to $\frac{G'}{G}$, &c.; then, supposing w unaltered, the variations of the quotients were found, which corresponded to changes of 1 in x, y and z; from these, by solving three linear equations, the corrections to x, y and z were found; and then a common multiplier for w and x was found by comparing the result of each corrected substitution with the tidal numbers G, G′, &c.

The other method was, to put the equations in the following form:

$$w\times(A+C.y+D.z)+x\times(B+E.y+F.z)=G.$$

Between two of the equations, w and x were eliminated, and a complicated equation between y and z remained; another equation of the same character was obtained from the other two of the original equations; and these two equations were solved by trials.

By these methods (but principally by the former) the equations were solved for $\alpha=0$ and $\alpha=-2^h$ for all the stations as far as Mullaghmore. Beyond that station it was found totally impracticable to solve them. Values of w, x, y, z were sometimes found which seemed nearly to satisfy the equations, but when an attempt was made to correct these values, the corrections became absurdly large, and the corrected values gave results much further from the truth than the original results. And for those stations at which the operation was successful, there were special results of inadmissible character. Thus, when $\alpha=0$, $\frac{M}{S}$ was found $=4{\cdot}30$ for Kilbaha, and $=1{\cdot}45$ for Mullaghmore; when $\alpha=-2^h$, $\frac{M}{S}$ was found $=3{\cdot}40$ for Kilbaha and $=0{\cdot}82$ for Mullaghmore. These discordances seemed to show that α must be positive; but in no case could a solution be obtained with a positive value for α.

On examining carefully the numbers given by observation, I was led to the following considerations, which seemed likely to throw considerable light on the subject.

On inspecting the table in page 17, it will be evident that at the first stations, as far as Old Head, the disappearance of diurnal tide at high water does not occur on the same day as the disappearance of diurnal tide at low water; the former always occurring earlier than the latter. But at the stations from Glenarm to Dunmore East, the disappearance of diurnal tide at high water sometimes precedes and sometimes follows that at low water; and may be said, roughly speaking, to occur on the same day. This circumstance fixes absolutely the value of α. For, when the diurnal tide at high water and that at low water vanish at the same time, the inference is, that at that time the lunar diurnal tide and the solar diurnal tide have equal values

with opposite signs four times in their diurnal period. If then their phases at the first of those four times be represented by φ and ψ respectively, and their coefficients by μ and ν, we have

$$\mu \sin \varphi + \nu \sin \psi = 0, \qquad \mu \sin (\varphi + 90°) + \nu \sin (\psi + 90°) = 0,$$

$$\mu \sin (\varphi + 180°) + \nu \sin (\psi + 180°) = 0, \quad \mu \sin (\varphi + 270°) + \nu \sin (\psi + 270°) = 0.$$

The solution of these equations is either $\mu = -\nu$, $\varphi = \psi$; or $\mu = \nu$, $\varphi = \psi + 180°$. Confining our expressions to the former (by which we lose nothing of generality), we have this result; that, upon that day, the solar diurnal tide and the lunar diurnal tide are in the same phases at every part of the day. Observing then that α is the angle which is to be added to the moon's hour-angle west, in order to give it the same relation to the phase of lunar diurnal tide which the sun's hour-angle west has to the phase of solar diurnal tide, it will be seen that α must be equal to the excess of the moon's right ascension over the sun's right ascension (altered by 12^h if necessary) on the day on which the diurnal tide vanishes both at high and low water.

In order to investigate the value of α with accuracy, the following process was used:—If from the table in page 17 we form the numbers "day of evanescence at low water — day of evanescence at high water," at Glenarm, or Donaghadee, &c., it will be seen that the four numbers at the same station have values alternately greater and less. This is owing, I conceive, to parallax, or some other cause which is periodical in one revolution of the moon nearly; and a correction is probably necessary, applicable with opposite signs to the alternate values. Thus, comparing the second with the mean of the first and third, half the difference is one value of the correction; comparing the third with the mean of the second and fourth, half the difference is another value of the correction; and the mean of these may be used. Thus corrected values of the "day of evanescence at low water — day of evanescence at high water" were obtained. Taking the means of the corresponding corrected values for the six stations from Glenarm to Dunmore East, we have,

About July .	$4^d \cdot 41$,	the mean value is	$0^d \cdot 76$,	and the excess of the moon's R.A. over the sun's R.A. is	$20^h\ 46^m$
About July .	17·86,	the mean value is	0·73	„ „ „	8 33
About August	1·92,	the mean value is	0·47	„ „ „	19 58
About August	14·56,	the mean value is	0·00	„ „ „	7 13

From the regularity of the progress of the numbers in the second and third columns, it appears certain that the value $7^h\ 13^m$ for α must be very near the truth.

From the reasoning above it will appear that, in the case of simultaneous evanescence of diurnal tide at high water and at low water, we have no means whatever of ascertaining the values of μ and φ on that day. Or, if we take the expressions on page 22, we have for diurnal tide at high water,

$$a.\sin\beta.\sin\{\text{☾} - \odot + A\} + b.\sin\{\text{☾} - \odot + B\},$$

and at low water,

$$a.\cos\beta.\sin\{\text{☾} - \odot + A\} + b.\cos\{\text{☾} - \odot + B\};$$

and, on the same day when both vanish,

$$\beta \text{ must} = \text{☾}' - \odot' + \text{B} \text{ and } a\sin\{\text{☾}' - \odot' + \text{A}\} \text{ must} = -b;$$

by which the expression for diurnal tide at high water is changed to

$$a.\sin\{\text{☾}' - \odot' + \text{B}\}.\sin\{\text{☾} - \odot + \text{A}\} - a.\sin\{\text{☾}' - \odot' + \text{A}\}.\sin\{\text{☾} - \odot + \text{B}\}$$
$$= a.\sin(\text{B} - \text{A}).\sin\{\overline{\text{☾} - \odot} - \overline{\text{☾}' - \odot'}\},$$

and that at low water is changed to

$$a.\cos\{\text{☾}' - \odot' + \text{B}\}.\sin\{\text{☾} - \odot + \text{A}\} - a.\sin\{\text{☾}' - \odot' + \text{A}\}.\cos\{\text{☾} - \odot + \text{B}\}$$
$$= a.\cos(\text{B} - \text{A}).\sin\{\overline{\text{☾} - \odot} - \overline{\text{☾}' - \odot'}\}.$$

The maximum of the compound diurnal tide at high water will then be $a.\sin(\text{B}-\text{A})$, and that at low water $a.\cos(\text{B}-\text{A})$. From the observed values of these maxima we may obtain the values of a and $\text{B}-\text{A}$; but we cannot from these obtain either A or B, or $a.\sin\beta$, or b; and thus the separation of the solar and lunar diurnal tides is in this case impossible.

As it seemed probable that the same value of α ($7^{\text{h}}\ 13^{\text{m}}$), which was inferred from these stations, would apply to the other stations also, I changed the equations for w, x, y, z, to the form corresponding to $\alpha = 7^{\text{h}}\ 13^{\text{m}}$. But, on attempting to solve them for Kilbaha, Kilrush, &c., I was entirely baffled. I could not in any instance approach to a solution.

As a last resource I resorted to the following method. Although the observations cannot be supposed competent to furnish more than four unknown quantities, yet they may be combined, and with sufficiently favourable coefficients, for the determination of the six quantities w, x, wy, wz, xy, xz: as the equations will be simple, values can certainly be found for these quantities; and then the accuracy of the results will be tested by ascertaining whether the following equations hold; $\frac{wy}{w} = \frac{xy}{x}$, $\frac{wz}{w} = \frac{xz}{x}$. In this manner then equations were formed and solved for the principal stations; but at none of them was there any approach towards satisfying the equations of condition which I have just given; nor even upon reducing the results to a common phase of the diurnal tide-wave, by means of the intervals of diurnal establishment to be given in Section IX., and taking the mean, was there any approach towards satisfying those equations. This is the last attempt that I made, and I confess myself, on the whole, completely unsuccessful.

The following considerations may perhaps explain the failure of all these attempts. We have seen that when the days of evanescence, of diurnal tide in semidiurnal high water, and of diurnal tide in semidiurnal low water, coincide, it is absolutely impossible to extract from the equations a result relating to the distinction of solar and lunar effects. It may therefore be inferred that, when the days of evanescence *nearly* coincide, the determination of the quantities sought will be *nearly* impossible, or will be liable to very great errors. Now, at all the stations the interval between

the days of evanescence for semidiurnal high water and for semidiurnal low water is small, and therefore it appears that in the case of the Irish tides in question, the difficulty of separating the solar and lunar effects is inevitable.

A treatment of the phenomena of diurnal tide which does not lead to a distinction of the solar and lunar effects must be considered as imperfect. And it has been explained that, though from a short series of observations the distinction cannot be extracted, it can with certainty be obtained from a long series of observations. With a full avowal of the completeness of my own failure and with a statement at the same time of what science requires and what it may reasonably expect, I may be permitted to explain an expression which I have used in my tract on Tides and Waves, Art. 564, where I have said that a determination by Mr. WHEWELL is worth little. My intention was to express that the distinction of the solar and lunar effects was theoretically important, that it might be obtained from the observations there referred to, and that it was not obtained. In stating that Mr. WHEWELL's determination was worth little, my expression was thus far incorrect, that a general rule for the order of diurnal tides, though liable to some inaccuracy, was really obtained by Mr. WHEWELL. I regret that I should have made a statement which could thus seem to be insufficiently founded, and still more that I should have expressed it in a phrase which could be interpreted as lightly esteeming the deductions of the writer to whom we are indebted more than to any other for the knowledge which we possess regarding the laws of the tides.

Section V.—*Discussion of the height of apparent mean water, as deduced from the heights of high and low water only, corrected for diurnal tide; with reference to difference of station, and to variations of the magnitude of the tide and of the moon's declination.*

The heights at high and low water were corrected for the diurnal tide in height found in Section III. The age of the semidiurnal tide as deduced from heights having been found (by the process of the next section) to be about two days, the times were taken from the Nautical Almanac, at which the moon's hour-angle from the sun was 3^h, 9^h, 15^h, 21^h, and two days being added to these, the times were determined which were to be used as separating the large tides from the small tides. The groups of observations thus marked off were the following:—

	d	h		d	h	
1st Group, June	28	6	to July	6	16	small tide.
2nd Group, July	6	16	to July	13	3	large tide.
3rd Group, July	13	3	to July	20	9	small tide.
4th Group, July	20	9	to July	28	9	large tide.
5th Group, July	28	9	to August	5	0	small tide.
6th Group, August	5	0	to August	11	16	large tide.
7th Group, August	11	16	to August	18	15	small tide.

For each of these groups the mean of the heights at high water was taken, and the mean of the heights at low water was taken. Then the mean of the determinations of each of these classes in the 1st, 3rd, 5th, and 7th groups was taken, and thus was obtained a mean height at high water and a mean height at low water in small tides. The mean of these two means gave the Apparent Mean Height in small tides (so called in order to distinguish it from the Mean Height which will be found in Section XI.). By a similar treatment of the 2nd, 4th, and 6th groups, the mean height at high water, mean height at low water, and Apparent Mean Height, in large tides, were found. The results are as follows :—

Station.	Small Tides.			Large Tides.			Mean of Apparent Mean Heights.	Excess of Apparent Mean Height in large tides above Apparent Mean Height in small tides.
	Mean height at high water.	Mean height at low water.	Apparent Mean Height.	Mean height at high water.	Mean height at low water.	Apparent Mean Height.		
	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.
Kilbaha	19·34	11·92	15·63	21·79	9·57	15·68	15·65	+0·05
Kilrush	20·50	12·06	16·28	22·96	9·77	16·37	16·32	+0·09
Foynes Island....	21·82	11·68	16·75	24·39	9·13	16·76	16·75	+0·01
Limerick.....	23·22	9·99	16·61	26·38	7·40	16·89	16·75	+0·28
Casleh Bay.....	21·81	14·12	17·97	24·43	11·52	17·98	17·97	+0·01
Galway	21·54	13·42	17·48	24·34	10·70	17·52	17·50	+0·04
Old Head	21·18	14·28	17·73	23·60	12·04	17·82	17·77	+0·09
Mullaghmore	21·22	14·78	18·00	23·42	12·92	18·17	18·08	+0·17
Buncrana	20·68	13·90	17·29	23·23	11·47	17·35	17·32	+0·06
Port Rush	18·95	16·23	17·59	20·35	15·13	17·74	17·66	+0·15
Carrowkeel	20·00	15·87	17·94	21·66	14·72	18·19	18·06	+0·25
Ballycastle	18·39	16·48	17·44	19·17	16·21	17·69	17·56	+0·25
Glenarm	20·08	15·11	17·60	20·60	14·67	17·64	17·63	+0·04
Donaghadee	21·73	13·83	17·78	23·12	12·56	17·84	17·81	+0·06
Ardglass.....	22·54	12·64	17·59	24·64	10·68	17·66	17·62	+0·07
Clogher Head....	21·85	12·30	17·08	23·91	10·28	17·10	17·09	+0·02
Kingstown	21·04	13·69	17·37	22·59	12·05	17·32	17·34	−0·05
Dunmore East ..	19·72	11·82	15·77	21·57	10·08	15·83	15·80	+0·06
New Ross	21·23	11·92	16·58	23·31	10·42	16·87	16·72	+0·29
Passage West....	20·07	11·67	15·87	22·01	10·15	16·08	15·97	+0·21
Castle Townsend..	18·90	12·22	15·56	20·44	10·87	15·66	15·61	+0·10

The column which first demands our attention is the "Mean of Apparent Mean Heights." The Apparent Mean Height increases in ascending the Shannon from Kilbaha to Limerick, and in ascending the Barrow from Dunmore East to New Ross. There is no difficulty in perceiving that such an effect must result from the mixture of a river-current with a tide, when the motion of the water is impeded by friction. But the Apparent Mean Height increases also from Kilbaha and Castle Townsend in going northward along both the eastern and the western coasts. And this occurs both at large tides and at small tides, with comparatively very little difference of amount. This conclusion will be fully supported by the results deduced by a more elaborate process from the whole mass of observations in Section XI. The details of levelling in the first section will scarcely permit us to ascribe any large part of this seeming difference of Apparent Mean Height to error in the instrumental process.

I regard this as a most important result, and I cannot with confidence offer any explanation of it.

The last column seems to show that the Apparent Mean Height is greater for large ranges of tide than for small ones, or that in spring tides the increase of elevation of the high water is greater than the increase of depression of the low water. A similar result has been found in the Thames. It does not appear to admit of explanation with our present theoretical knowledge. But in a period of two months it is impossible to separate the effects of varying range from those of varying declination of the moon and other varying circumstances.

For investigation of the effect of the moon's declination on the apparent mean height, the following process was used. It is to be remarked that (setting aside the differences of the moon's distance) the effect of southern declination is the same as that of northern declination; and the effect is proportional nearly to the square of declination; and therefore the groups into which our observations are to be divided are to be classified by large declinations (of either kind) and by small declinations; the separating time being nearly that at which the moon's declination $=\frac{1}{\sqrt{2}}\times$ moon's maximum declination. The times being thus found, one day was added to each, and thus the following separation of groups was made.

	d	h		d	h	
1st Group, June	26	3	to July	3	14,	small declination.
2nd Group, July	3	14	to July	10	2,	large declination.
3rd Group, July	10	2	to July	16	8,	small declination.
4th Group, July	16	8	to July	23	10,	large declination.
5th Group, July	23	10	to July	30	22,	small declination.
6th Group, July	30	22	to August	6	12,	large declination.
7th Group, August	6	12	to August	12	15,	small declination.
8th Group, August	12	15	to August	19	16,	large declination.

For each of these groups, the mean of the heights at high water was taken, and the mean of the heights at low water; and the mean of these gave a mean Apparent Mean Height in each group. The mean of the results from the 1st, 3rd, 5th and 7th groups, was used for small declinations, and the mean of the results from the 2nd, 4th, 6th and 8th, was used for large declinations. At Ballycastle the first group was wanting. Thus the following Table was found.

Station.	Apparent Mean Height with small declination.	Apparent Mean Height with large declination.	Excess of Apparent Mean Height with large declination above Apparent Mean Height with small declination.
	ft.	ft.	ft.
Kilbaha	15·64	15·66	+0·02
Kilrush	16·29	16·35	+0·06
Foynes Island .	16·71	16·81	+0·10
Limerick.....	16·70	16·79	+0·09
Casleh Bay....	17·94	18·00	+0·06
Galway	17·46	17·53	+0·07
Old Head	17·75	17·79	+0·04
Mullaghmore ..	18·05	18·13	+0·08
Buncrana	17·31	17·38	+0·07
Port Rush	17·64	17·70	+0·06
Carrowkeel....	17·98	18·11	+0·13
Ballycastle	17·51	17·60	+0·09
Glenarm.....	17·56	17·69	+0·13
Donaghadee ..	17·77	17·86	+0·09
Ardglass.....	17·59	17·70	+0·11
Clogher Head..	17·00	17·18	+0·18
Kingstown	17·26	17·45	+0·19
Dunmore East .	15·77	15·82	+0·05
New Ross	16·72	16·69	−0·03
Passage West..	15·99	15·93	−0·06
Castle Townsend	15·60	15·59	−0·01

In like manner, a series of groups was formed, determined by the increase or decrease of the moon's declination without regard to its sign; one day being added to each of the times when the moon's declination was 0, and when it was maximum either north or south. Thus the following separation of groups was made.

	d	h		d	h	
1st Group, June	22	11	to June	29	21,	declination decreasing.
2nd Group, June	29	21	to July	7	0,	declination increasing.
3rd Group, July	7	0	to July	13	3,	declination decreasing.
4th Group, July	13	3	to July	19	19,	declination increasing.
5th Group, July	19	19	to July	27	4,	declination decreasing.
6th Group, July	27	4	to August	3	9,	declination increasing.
7th Group, August	3	9	to August	9	12,	declination decreasing.
8th Group, August	9	12	to August	16	0,	declination increasing.
9th Group, August	16	0	to August	23	11,	declination decreasing.

The mean of results from the 1st, 3rd, 5th, 7th and 9th groups was used for decreasing declination, and that of results from the 2nd, 4th, 6th and 8th, for increasing declination. At the first six stations, and at Ballycastle and Passage West, the first group was deficient. Thus the following Table was formed.

Station.	Apparent Mean Height with decreasing declination.	Apparent Mean Height with increasing declination.	Excess of Apparent Mean Height with decreasing declination over Apparent Mean Height with increasing declination.
	ft.	ft.	ft.
Kilbaha	15·65	15·64	+0·01
Kilrush	16·37	16·27	+0·10
Foynes Island .	16·76	16·74	+0·02
Limerick.....	16·84	16·64	+0·20
Casleh Bay....	17·96	17·95	+0·01
Galway	17·50	17·47	+0·03
Old Head	17·85	17·37	+0·48
Mullaghmore ..	18·23	17·99	+0·24
Buncrana	17·47	17·27	+0·20
Port Rush	17·82	17·57	+0·25
Carrowkeel....	18·19	17·92	+0·27
Ballycastle	17·68	17·45	+0·23
Glenarm.....	17·73	17·59	+0·14
Donaghadee ..	17·91	17·77	+0·14
Ardglass.....	17·72	17·59	+0·13
Clogher Head..	17·17	17·05	+0·12
Kingstown	17·41	17·35	+0·06
Dunmore East .	15·87	15·76	+0·11
New Ross	16·85	16·61	+0·24
Passage West ..	16·05	15·89	+0·16
Castle Townsend	15·65	15·57	+0·08

It would seem from this that, if the apparent mean height depends upon the moon's declination, it is greatest when the moon's declination has decreased nearly to the point where the argument of declination is 135°, or 315° (which makes the declination $=\frac{1}{\sqrt{2}}\times$ maximum declination). Thus at the middle of the observations, it is greatest about July 22. Now on July 22, the magnitude of the tide, though above the mean, was not maximum. From this circumstance, and from the decided superiority in magnitude of the excess found by classifying the tides by the moon's declination, over the excess found by classifying the tides by the magnitude of the tide, it appears extremely probable that the excess does really depend on the moon's declination. In that case, the greatest apparent mean height will occur about four days after the day of greatest declination.

Section VI.—*Discussion of the range of tide; and of semimenstrual inequality in height, apparent proportion of solar and lunar effects as shown by heights, and age of tide as shown by heights; from high waters and from low waters.*

The numbers from which we shall extract the results of this section are contained in the table of page 30. The means of the heights are made subservient to the accurate determinations of specific heights in the following manner. If θ is the difference of right ascension of the sun and moon, S and M their respective single effects, then (neglecting declinations, &c.) the actual height may be represented by $A+M\sqrt{\left\{1+\frac{2S}{M}\cos 2\theta+\frac{S^2}{M^2}\right\}}$. If we expand this and integrate from $\theta=-45°$ to

$\theta = +45°$, and divide by $\frac{\pi}{2}$, we obtain the expression for the mean of large tides. If we integrate from $\theta = 45°$ to $\theta = 135°$, and divide by $\frac{\pi}{2}$, we obtain the expression for the mean of small tides. The difference between these, which will be found to be $\frac{4}{\pi}S - \frac{2}{3\pi}\cdot\frac{S^3}{M^2}$, is the same as the difference between the second and fifth columns in the table of p. 30, if high water is under consideration, or as the difference between the third and sixth columns, if low water is under consideration, or as the difference between the 2nd column $-$3rd, and 5th column $-$6th, if the range is considered. The second term of the formula may sometimes be omitted: and then we have $\frac{4}{\pi}S =$ difference, and $S = \frac{\pi}{4}\times$ difference, S being the solar effect in the elevation or range under consideration. If we add this to the mean elevation, which is represented by A+M nearly, we shall have A+M+S which is the absolute maximum; and if we subtract it, we have A+M$-$S, which is the absolute minimum. The same applies to range, with this difference only, that the constant A will be eliminated in subtracting the heights at low water from those at high water.

In order to obtain the value of $\frac{S}{M}$, we may remark that the difference of the two means divided by half the sum is $\frac{4\frac{S}{M} - \frac{2}{3}\frac{S^3}{M^3}}{\pi\left(1+\frac{1}{4}\frac{S^2}{M^2}\right)}$; and if we call this δ, we easily find $\frac{S}{M} = \frac{\pi}{4}\delta\left\{1+\frac{5}{12}\left(\frac{\pi}{4}\delta\right)^2\right\}$, in which the last term is small.

Neither of these expressions is perfectly correct, because they assume that the tidal day is always of the same length.

By means of these formulæ, and the numbers on page 30, the following Table is formed.

Station.	Difference of mean heights in large tides and in small tides.		Proportion of difference at high water to difference at low water.	Range in mean of large tides.	Range in mean of small tides.	Difference.	Mean range, or M.	Difference divided by mean range.	Corresponding value of $\frac{S}{M}$.	Absolute maximum range.	Absolute minimum range.	Half difference, or S.
	High water.	Low water.										
	ft.	ft.	ft.	ft.	ft.	ft.	ft.			ft.	ft.	ft.
Kilbaha	2·45	2·35	1·04	12·22	7·42	4·80	9·82	0·49	0·41	13·8	5·8	4·0
Kilrush	2·46	2·29	1·07	13·19	8·44	4·75	10·82	0·44	0·36	14·7	6·9	3·9
Foynes Island .	2·57	2·55	1·01	15·26	10·14	5·12	12·70	0·40	0·33	16·9	8·5	4·2
Limerick.....	3·16	2·59	1·22	18·98	13·23	5·75	16·11	0·36	0·29	20·8	11·4	4·7
Casleh Bay....	2·62	2·60	1·01	12·91	7·69	5·22	10·30	0·51	0·43	14·7	5·9	4·4
Galway	2·80	2·72	1·03	13·64	8·12	5·52	10·88	0·51	0·43	15·6	6·1	4·7
Old Head	2·42	2·24	1·08	11·56	6·90	4·66	9·23	0·50	0·42	13·1	5·3	3·9
Mullaghmore ..	2·20	1·86	1·18	10·50	6·44	4·06	8·47	0·48	0·40	11·9	5·1	3·4
Buncrana	2·55	2·43	1·05	11·76	6·78	4·98	9·27	0·54	0·46	13·6	5·0	4·3
Port Rush	1·40	1·10	1·27	5·22	2·72	2·50	3·97	0·63	0·55	6·0	1·8	2·2
Carrowkeel....	1·66	1·15	1·44	6·94	4·13	2·81	5·54	0·51	0·43	7·9	3·1	2·4
Ballycastle	0·78	0·27	2·90	2·96	1·91	1·05	2·44	0·43	0·36	3·3	1·5	0·9
Glenarm	0·52	0·44	1·18	5·93	4·97	0·96	5·45	0·18	0·14	6·2	4·7	0·8
Donaghadee ..	1·39	1·27	1·10	10·56	7·90	2·66	9·23	0·29	0·23	11·3	7·1	2·1
Ardglass.....	2·10	1·96	1·07	13·96	9·90	4·06	11·93	0·34	0·27	15·1	8·7	3·2
Clogher Head..	2·06	2·02	1·02	13·63	9·55	4·08	11·59	0·35	0·28	14·8	8·4	3·2
Kingstown	1·55	1·64	0·95	10·54	7·35	3·19	8·95	0·36	0·29	11·6	6·4	2·6
Dunmore East .	1·85	1·74	1·06	11·49	7·90	3·59	9·70	0·37	0·30	12·6	6·8	2·9
New Ross	2·08	1·50	1·39	12·89	9·31	3·58	11·10	0·32	0·26	14·0	8·2	2·9
Passage West..	1·94	1·52	1·28	11·86	8·40	3·46	10·13	0·34	0·27	12·8	7·4	2·7
Castle Townsend	1·54	1·35	1·14	9·57	6·68	2·89	8·13	0·36	0·29	10·5	5·7	2·4

The numbers in this table present several subjects for our consideration.

First, it is to be remarked that the difference between the numbers of the second and third columns corresponds to the number in the last column of the table on page 30 (it is, in fact, the double of that number); but the exhibition of the numbers in this form serves, in some instances, to point out their origin more distinctly. At Limerick, for instance, it appears that a large tide produces less effect on the low water than on the high water; and the reason evidently is, that the height of low water there depends more upon the freshwater current of the river Shannon, than upon the state of the water in the sea, whereas the height of high water depends mainly on the sea. The same explanation applies at New Ross and at Passage West. It does not however apply to Mullaghmore, Port Rush, Ballycastle, or Glenarm; nor, with perfect certainty, to Carrowkeel. I must refer to Section X. and some of the succeeding sections, for a statement of the laws of the individual tides at these places; and shall only remark here, that much remains to be done with the theoretical treatment of the motion of waves, before the tides on the north-eastern coast can be fully explained.

The next column which deserves particular attention is that for $\frac{S}{M}$. From the agreement among the numbers at the littoral stations on the western coast (Kilbaha, Casleh Bay, Galway, Old Head, Mullaghmore), it seems certain that the value of $\frac{S}{M}$, in this part of the Atlantic, is pretty exactly 0·42. At Brest, in a position equally

exposed to the open sea, LAPLACE found $\frac{S}{M}=0{\cdot}33$. I imagine that, as regards the sun's position in declination, the present series of tides may be supposed to give a value for $\frac{S}{M}$ differing very little from the mean, but rather too small than too great. If so, here is an undoubted discordance. Yet it is remarkable that the series of numbers in this table presents a still greater discordance under circumstances where we should hardly expect it. The littoral station of Castle Townsend is fully open to the Atlantic; and those of Dunmore East, Kingstown, and Clogher Head, are more and more subject to the effects (whatever they may be) of the inclosure of the Irish Channel. Yet all these stations, including Castle Townsend, give for $\frac{S}{M}$ a value of about 0·28.

In speculating on the causes of these discordances, it is important to observe, that the relation of the successive changes of a progressive tide may be considered in two different ways. One of these ways is applicable properly to rivers and similar channels; it consists in assuming that each semidiurnal tide is nearly independent of every other semidiurnal tide, and that the quantities of the second and higher orders of the range are sensible*. The other is applicable to open seas, where the vertical oscillation is insignificant in comparison with the depth, and where the alteration of the horizontal surface, by the shoaling of the shore, is perfectly unimportant; here the principle of the superposition of small movements applies, and the solar and lunar tides may be considered as perfectly independent. Thus considered, the discordance to which I have alluded may be stated thus: the lunar tide at Castle Townsend is less than that at Kilbaha, in the proportion of 8·13 to 9·82, but the solar tide at Castle Townsend is less than that at Kilbaha, in the proportion of 2·4 to 4·0. There can be no doubt that in the lunar tide, the difference between 8·13 and 9·82 depends (among other circumstances) upon the periodic time of the tide, inasmuch as a tide infinitely slow would produce the same effect at both stations; and therefore we must not be surprised that the solar tide, whose recurrence is more rapid than that of the lunar tide, should undergo a larger proportional alteration, like that between 2·4 and 4·0.

In another section I shall show that between Dunmore East and Kingstown, the tide is suddenly and completely reversed, high water at one of these stations corresponding exactly (in time) to low water at the other; while from Dunmore East to Donaghadee, there is scarcely any perceptible difference of phase. It is worthy of remark, that during these changes and coincidences, the value of $\frac{S}{M}$ is unaltered.

The remarkable change in the values of $\frac{S}{M}$ at Port Rush, Ballycastle, and Glenarm, depends also, without doubt, upon the difference of the modifications produced in the

* Encyclopædia Metropolitana, *Tides and Waves*, Section IV., Subsection 3.

solar and in the lunar tides, by the narrow channel which, beginning with Islay (opposite Port Rush), continues to the Mull of Galloway (opposite Donaghadee). But I profess myself totally unable to explain with greater accuracy why, in passing from Mullaghmore to Ballycastle, the principal reduction of lunar tide takes place between Mullaghmore and Port Rush, while the principal reduction of solar tide takes place between Port Rush and Ballycastle.

The successive diminution in the values of $\frac{S}{M}$ from Kilbaha to Kilrush, Foynes Island, and Limerick, as well as the reduction from Dunmore East to New Ross, and that from Port Rush to Buncrana and Carrowkeel, are to be explained from the other consideration which I have described as applicable to rivers. In fact, in these cases, from the very greatly contracted current-way at low water, the effect of variation in the depression of the ocean low water is very small, and therefore the effect of semi-menstrual inequality is sensible almost only in the high water, and its whole effect on the range is therefore less than it ought to be.

Before dismissing this table, I shall remark, that the very great diminution in the range of tide in the North Channel, makes it impossible for us to believe that the tide in the Irish Channel is supplied from the North Channel, however much the consideration of the times in the next section may lead us, in the first instance, to imagine so.

I will now proceed with the age of the tide as shown by the heights. The method employed was the following:—

The mean heights having been found from the preceding tables, the heights for every day (corrected for diurnal tide) were examined, and the time was ascertained as nearly as practicable at which the height coincided with the mean height. These times were then compared with the times at which the moon's hour-angle from the sun was 3^h, 9^h, 15^h, and 21^h; namely, June, $26^d\ 6^h$; July, $4^d\ 16^h$, $11^d\ 3^h$, $18^d\ 9^h$, $26^d\ 9^h$; August, $3^d\ 0^h$, $9^d\ 16^h$, $16^d\ 15^h$. High waters and low waters were treated separately. At each place eight comparisons were obtained for high water and eight for low water: excepting that in the high waters, no comparison was made at Old Head with July $11^d\ 3^h$, and $18^d\ 9^h$ (the times of mean height being very uncertain); nor at Ballycastle, with July $18^d\ 9^h$ and $26^d\ 9^h$ (for the same reason), nor with June $26^d\ 6^h$ (the observations having commenced too late); nor at Glenarm, Donaghadee, or Kingstown, with July $18^d\ 9^h$ and $26^d\ 9^h$ (the times being uncertain). The results for each place being collected, and the means being taken, the following Table was formed:—

Age of Tide as inferred from Heights.

Station.	High water.	Low water.	Station.	High water.	Low water.
	d h	d h		d h	d h
Kilbaha	2 2	2 0	Ballycastle	2 2	2 17
Kilrush	1 21	2 3	Glenarm.....	1 7	2 6
Foynes Island....	2 2	2 4	Donaghadee	1 19	2 17
Limerick.....	2 2	2 7	Ardglass.....	2 5	2 11
Casleh Bay.....	2 0	2 2	Clogher Head....	2 4	3 4
Galway	2 2	2 2	Kingstown	1 20	2 13
Old Head	1 15	2 6	Dunmore East ..	2 1	2 14
Mullaghmore....	1 22	2 8	New Ross	2 2	2 14
Buncrana	2 1	2 5	Passage West....	1 21	2 7
Port Rush	1 17	2 5	Castle Townsend.	1 21	2 9
Carrowkeel.....	1 17	2 6			

In the Encyclopædia Metropolitana, *Tides and Waves*, Art. 545, I have given reasons for believing that the true age of the tide is that given by the heights. From the inspection of this table it appears certain that the age of the tide on the western coast of Ireland is almost exactly two days. The different results are upon the whole very consistent. Yet there are some discordances which I cannot entirely explain. The most distinctly marked discordance is this—that the age given by the Low Waters is, in every instance except one, greater than that given by the High Waters. I have shown, I think, with certainty, in the Encyclopædia Metropolitana, *Tides and Waves*, Articles 452 and 544, that the cause of the principal part of the apparent age of tide is friction. It appears to me not unlikely, that, on carrying out the theory of friction in combination with the consideration of oscillations bearing a finite proportion to the depth of the sea, the apparent age of the tide might be found greater for low waters than for high waters. But I have not examined the theory so far as to feel myself warranted in positively assigning this as the explanation.

Section VII.—*Establishment of each port, and progress of semidiurnal tide round the island.*

The process used in this investigation is very nearly similar to that by which the table in page 30 was formed. The mean interval from the moon's transit at Greenwich to the time of tide was found (by the operations of the next section) to coincide with the actual interval nearly at the following times:—Port Rush and Ballycastle (high and low), June, $22^d\ 9^h$, $30^d\ 20^h$; July, $7^d\ 19^h$, $14^d\ 21^h$, $21^d\ 23^h$, $30^d\ 16^h$; August, $6^d\ 4^h$, $13^d\ 5^h$, $20^d\ 16^h$: Kilbaha, Kilrush, Casleh Bay, Galway, Mullaghmore, Buncrana, Carrowkeel, Glenarm, Donaghadee, Ardglass, Clogher Head, Kingstown, Castle Townsend, (high and low), $1^d\ 8^h$ later; Foynes Island, Old Head, Dunmore East, Passage West (high and low), Limerick (high only), 20^h later than the last; New Ross (high and low), 23^h later than the last; Limerick (low only), 22^h later than the last, or $4^d\ 1^h$ later than at Port Rush and Ballycastle. The intervals from the moon's transit to the time of tide being taken for every tide (the high water, 1st division, being referred to the upper transit, the low water, 1st division, to the transit over the

meridian 6^h west, the high water, 2nd division, to the lower transit, and the low water, 2nd division, to the transit over the meridian 18^h west), groups were formed divided by the times above mentioned, and the mean of the intervals was taken for each group. Thus the following Table was formed:—

Station.	High water.			Low water.			Mean of both.
	Mean of large intervals.	Mean of small intervals.	Mean.	Mean of large intervals.	Mean of small intervals.	Mean.	
	h m	h m	h m	h m	h m	h m	h m
Kilbaha	5 11	4 23	4 47	5 6	4 12	4 39	4 43
Kilrush	5 22	4 31	4 56	5 20	4 28	4 54	4 55
Foynes Island....	6 15	5 30	5 52	6 0	5 12	5 36	5 44
Limerick.....	6 46	6 0	6 23	7 14	6 17	6 45	6 34
Casleh Bay.....	5 21	4 30	4 55	5 11	4 14	4 42	4 48
Galway	5 28	4 40	5 4	5 11	4 13	4 42	4 53
Old Head	5 22	4 47	5 4	5 22	4 45	5 3	5 4
Mullaghmore....	6 2	5 9	5 35	5 53	5 7	5 30	5 33
Buncrana	6 45	5 39	6 12	6 34	5 36	6 5	6 8
Port Rush	7 29	6 0	6 45	7 14	5 59	6 36	6 40
Carrowkeel.....	8 11	7 2	7 36	8 1	7 4	7 32	7 34
Ballycastle	8 37	6 14	7 25	8 47	7 3	7 55	7 40
Glenarm.....	11 1	10 26	10 43	11 7	10 22	10 44	10 44
Donaghadee	11 21	10 42	11 1	11 23	10 36	11 0	11 1
Ardglass.....	11 21	10 40	11 0	11 25	10 38	11 2	11 1
Clogher Head....	11 41	10 54	11 17	11 34	10 47	11 10	11 14
Kingstown.....	11 52	11 6	11 29	11 29	10 41	11 5	11 17
Dunmore East ..	5 32	4 47	5 9	5 41	4 56	5 18	5 13
New Ross	6 17	5 36	5 56	6 55	6 9	6 32	6 14
Passage West....	5 38	4 55	5 16	5 52	5 9	5 30	5 23
Castle Townsend.	5 16	4 29	4 52	5 19	4 30	4 54	4 53

On inspecting the intervals for Castle Townsend, Kilbaha, Casleh Bay, Galway, Old Head, and Mullaghmore, it appears evident that the semidiurnal tide approaches the western coast of Ireland very nearly from the west, or possibly from a direction a little south of the west. On examining the intervals at Port Rush, Ballycastle, Glenarm, Donaghadee, their regular progression seems at first to show that the tide wave enters the Irish Sea by way of the North Channel. But the objections to this supposition are most serious. Theoretically it is certain that (even without regarding the effects of friction) when a tide-wave passing through a narrow channel enters into a wide one its vertical range will be diminished*. But here the mean range in the narrow channel at Ballycastle is only $2\frac{1}{2}$ feet and at Glenarm $5\frac{1}{2}$ feet: this tide-wave therefore could not possibly produce a tide of more than one or two feet in the Irish Sea; whereas, from Ardglass southward to Kingstown, the range is from ten to twelve feet. It seems therefore that this supposition may at once be dismissed; and the only supposition which can be substituted for it is, that the tide-wave enters the Irish Sea by St. George's Channel. But here a most remarkable circumstance occurs. A reference to the table above will show that the high water at Kingstown occurs six hours and a few minutes later than that at Dunmore East, or, in other words, that

* Encyclopædia Metropolitana, *Tides and Waves*, Art. 264.

the high water at Kingstown coincides *precisely* with the low water at Dunmore East, and *vice versâ*. Moreover, between these two stations occurs the station Courtown; and here, as will appear in Section XVII., the semidiurnal tide is nearly insensible. The difference in the times at Dunmore East and Kingstown does not therefore arise from a slow transmission of tide; but arises from a sudden *inversion* of the wave, the point which separates elevation from depression being not far from Courtown. And the question now is, whether, on the supposition that the tide-wave enters the Irish Sea by this southern entrance, it is possible to explain the existence of this neutral point and the inversion of the tide beyond it.

The explanation which I have to give rests upon a proposition long known to me as a matter of theory, but for which I never expected to find a practical application. In the Encyclopædia Metropolitana, *Tides and Waves*, Art. 307, I have shown that when the tidal wave enters a gulf (considered as a canal of uniform breadth and depth, stopped by a transverse barrier), the expression for the elevation of the water at the time t at a point whose distance from the sea is x, is $\frac{A}{\cos ma} \cdot \sin \overline{nt+B} . \cos \overline{ma-mx}$; where n is the constant proper to the periodic time of the tide-wave, $\frac{n}{m} = \sqrt{g \times \text{depth}}$, and a is the whole length of the canal. This expression shows that the vertical oscillations are simultaneous throughout; the coefficient in each place being $\frac{A}{\cos ma} \cos \overline{ma-mx}$. Now if the canal is so long and so shallow that ma is greater than $\frac{\pi}{2}$; then, on taking $x = a - \frac{\pi}{2m}$ the coefficient vanishes; and on taking x greater than $a - \frac{\pi}{2m}$ the coefficient changes its sign. This case then agrees, as regards the simultaneity and the inversion, with the case of observation before us, and all that is necessary for its complete application is, that the virtual head of the channel be supposed to be at a distance from Courtown equal to that which a *progressive* wave would pass over in $3^h\ 5^m$ $\left(\text{which would make } ma = \frac{\pi}{2}\right)$. Without professing myself able to enter into details upon the depth of the water and the form of the supposed channel, I express my belief that this solution does accurately apply here: and I regard it as one of the most remarkable cases that has ever been noticed in the observations of tides.

Assuming then that the tide of the Irish Sea is explained, we have only further to explain the apparent passage of the tide through the North Channel. This is merely a case of the proposition in Art. 310 of the *Tides and Waves*, relating to a canal joining two tidal seas, from which it appears that there will always be in such a canal an apparent passage of the tide-wave in the same direction at every part. To represent the circumstances completely, it would be necessary to introduce the consideration of friction.

The only remaining result of the table in page 39 which deserves attention here, is the difference of the times occupied by the rise of the water and by the fall of the water. In the river stations (Limerick and New Ross) the fall of the water occupies a longer time than the rise. This, as a consequence of theory, is explained in Art. 206 of the *Tides and Waves*. At most of the other stations, the rise appears to occupy a very little longer time than the fall. This, however, as it depends on the estimation of the times of high and low water, is subject to great doubt: the terms upon which such difference depends will be investigated with great accuracy in Sections X. and XVI.

Section VIII.—*Semimenstrual inequality in time, proportion of solar and lunar effects as shown by times, and apparent age of tide as shown by times; from high water and from low water.*

The interval from the moon's transit over the meridian to high water (and similarly from the moon's transit over the six-hour meridian to low water) is theoretically expressed by E+F, where E is a constant, and $\tan 2F=\frac{\frac{S}{M}\sin 2\theta}{1+\frac{S}{M}\cos 2\theta}$, θ being the hour-angle of the moon from the sun. The declinations, &c. are supposed here to have their mean values. Investigating from this the expression for F, integrating from $\theta=0$ to $\theta=\frac{\pi}{2}$, and dividing by $\frac{\pi}{2}$, we obtain the value of the mean of large intervals; performing the same operation from $\theta=\frac{\pi}{2}$ to $\theta=\pi$, we obtain the value of the mean of small intervals. The difference which will be found $=\frac{2}{\pi}\cdot\frac{S}{M}\cdot\left(1+\frac{1}{9}\left(\frac{S}{M}\right)^2\right)$, is the difference between the 2nd and 3rd columns or between the 5th and 6th columns in page 39, expressed in arc; or, as 2π of arc in the estimation of θ and F correspond to a tidal day of 1488^m, if we put i for the number of minutes in that difference, the equation is

$$\frac{2}{\pi}\cdot\frac{S}{M}\cdot\left(1+\frac{1}{9}\cdot\left(\frac{S}{M}\right)^2\right)=i\times\frac{2\pi}{1488}.$$

From this we obtain

$$\frac{S}{M}=\frac{\pi^2.i}{1488}\times\left\{1-\frac{1}{9}\cdot\left(\frac{\pi^2 i}{1488}\right)^2\right\}\text{ nearly;}$$

and then the maximum value of F in arc will be found by making $\sin 2F'=\frac{S}{M}$, and converting F' into time by the proportion stated above. Thus the following Table is formed.

Station.	High water.			Low water.			Mean of values of $\frac{S}{M}$.
	Difference of means of large intervals and small intervals.	Value of $\frac{S}{M}$.	Maximum semimenstrual inequality ±.	Difference of means of large intervals and small intervals.	Value of $\frac{S}{M}$.	Maximum semimenstrual inequality ±.	
	m		m	m			
Kilbaha	48	0·32	38·5	54	0·35	42	0·34
Kilrush	51	0·34	41	52	0·34	41	0·34
Foynes Island....	45	0·30	36	48	0·32	38·5	0·31
Limerick.....	46	0·31	37	62	0·40	48·5	0·35
Casleh Bay.....	51	0·34	41	57	0·37	44·5	0·36
Galway	48	0·32	38·5	58	0·37	44·5	0·34
Old Head	35	0·23	27	37	0·24	28·5	0·24
Mullaghmore....	53	0·35	42	46	0·31	37	0·33
Buncrana	66	0·42	51	58	0·37	44·5	0·39
Port Rush	89	0·57	71·5	75	0·49	60	0·53
Carrowkeel.....	69	0·45	54·5	57	0·37	44·5	0·41
Ballycastle	143	0·82	113	104	0·65	83	0·74
Glenarm.....	35	0·23	27	45	0·30	36	0·26
Donaghadee	39	0·25	30	47	0·31	37	0·28
Ardglass.....	41	0·27	32	47	0·31	37	0·29
Clogher Head....	47	0·31	37	47	0·31	37	0·31
Kingstown	46	0·31	37	48	0·32	38·5	0·31
Dunmore East ..	45	0·30	36	47	0·31	37	0·31
New Ross	41	0·27	32	46	0·31	37	0·29
Passage West....	43	0·28	33·5	43	0·28	33·5	0·28
Castle Townsend.	47	0·31	37	49	0·33	39·5	0·32

I have stated in the Encyclopædia Metropolitana, *Tides and Waves*, Art. 538, that I consider the values of $\frac{S}{M}$ deduced from the semimenstrual inequalities in time to be real and certain representations of the proportions of the sun's effect to the moon's effect in the seas in the neighbourhood of each station; those deduced from the heights being liable to the effects of many local disturbing causes which do not affect those deduced from the times. In this view the table above deserves consideration. The littoral stations (including those in the Irish Sea) agree in giving a value of 0·32 or 0·33, nearly the same as that found at Brest by Laplace and Sir J. W. Lubbock. But at Port Rush and Ballycastle (the first stations in the North Channel) the lunar tide appears to be diminished in a far greater proportion than the solar tide. And then, after this alteration of relative magnitude has been established in the open sea of the neighbourhood, it appears to be again nearly destroyed by some local cause affecting the heights, so that in the table of page 35, the value of $\frac{S}{M}$ is restored to its average value. As far as the observations can be trusted for accuracy, the two conclusions which I have mentioned appear at first sight perfectly certain; for the greatest difference of intervals from moon's transit (on which the value of $\frac{S}{M}$ in this page depends) is deduced from comparisons of the times of tides of equal vertical range, in which therefore the stream of tide in the neighbouring channels of small depth and width, &c. was the same, and therefore could not disturb the difference of times.

But the value in page 35 is deduced from the comparison of high and low tides, in which the stream of tides, &c. is different. The second apparent alteration can take place only where the tide has arrived at such localities that the second order of the vertical oscillation produces sensible terms. I was at first misled by the plausibility of this reasoning.

Its fallacy, or rather its error, will appear from the following considerations. The semimenstrual inequality in time which theoretically is proper for giving the value of $\frac{S}{M}$ is that which depends only upon those differences of time which are caused by difference in the relative positions of the sun and moon, when the magnitudes of the tides are exactly the same. But when there also exists a difference of time caused by the difference of magnitude of the tide (having its maximum nearly at the time of evanescence of the proper semimenstrual inequality), then these two differences or inequalities are combined, forming a single inequality whose time of evanescence is different from those of both the original inequalities, and whose magnitude is greater than the magnitude of either. Thus it appears that the gross semimenstrual inequality in time must not be used for estimation of $\frac{S}{M}$. A correct application of these principles, and a consequent harmony of results, will be seen in Section XIV.

At every station except Mullaghmore, the value of $\frac{S}{M}$ in the table above appears greater at low water than at high water. This evidently depends upon the difference in times (as affected by magnitude of tide) for low water and for high water, which is combined as above stated with the proper semimenstrual inequality.

Mr. WHEWELL, in his invaluable memoirs on cotidal lines, stated that there were great contradictions in the accounts of the establishment of Ballycastle. The numbers above serve in some degree to explain this. The semimenstrual inequality alone, comparing observations taken when its value was maximum positive with those taken when its value was maximum negative, would produce nearly four hours of uncertainty. More than half an hour (see the table in page 19) might be added by the diurnal tide.

I shall now proceed with the age of tide as shown by the times. The method employed was the same as for the heights, in forming the table in page 38. The times were ascertained at which the interval from moon's transit over the meridian to high water (and similarly the interval from moon's transit over the 6-hour meridian to low water), corrected for diurnal tide, agreed with the mean interval for high water (or for low water) in page 39. These times were then compared with the times at which the moon's hour-angle from the sun was 0^h, 6^h, 12^h, 18^h; namely, June, $22^d\ 9^h$, $30^d\ 20^h$; July, $7^d\ 19^h$, $14^d\ 21^h$, $21^d\ 23^h$, $30^d\ 16^h$; August, $6^d\ 4^h$, $13^d\ 5^h$, $20^d\ 16^h$. The high and low waters were treated separately. At Ballycastle low water six results only were obtained; for all the other determinations seven or eight results were compared. Thus the following Table was formed.

Age of Tide as inferred from Times.

Station.	High water.	Low water.	Station.	High water.	Low water.
	d h	d h		d h	d h
Kilbaha	1 8	1 5	Ballycastle	−0 11	−0 19
Kilrush	1 13	1 10	Glenarm	+1 14	1 9
Foynes Island....	1 21	2 2	Donaghadee	1 7	1 9
Limerick.....	1 22	4 1	Ardglass.....	1 8	1 15
Casleh Bay.....	1 14	0 22	Clogher Head....	1 10	1 12
Galway	1 10	1 7	Kingstown	0 22	0 23
Old Head	2 10	2 9	Dunmore East ..	2 11	2 8
Mullaghmore....	1 6	1 11	New Ross	2 20	3 11
Buncrana	1 4	1 3	Passage West....	2 2	1 19
Port Rush	0 4	0 7	Castle Townsend..	1 12	1 6
Carrowkeel.....	1 0	1 1			

In the *Tides and Waves*, Art. 463 and 465, I have shown that the age of tide inferred from the times of high water in a river (where spring-tide high waters pass more rapidly than neap-tide high waters) is too small, and that the age of tide inferred from the times of low water in a river (where spring-tide low waters pass more slowly than neap-tide low waters) is too great. These propositions, at least the second, are well illustrated at Limerick and New Ross. For the other stations I feel myself in some difficulty. With the exception only of Old Head, Dunmore East, and Passage West, all the ages of tide above are too small, for low water as well as for high water. This requires us to assume that all the phases of the tide-wave (low water as well as high water) are transmitted over the sea more rapidly in the spring-tides than in the neap-tides. I conjecture that some theory of friction may possibly explain this. It cannot be explained by supposing the second power of the small movements sensible; for on that assumption the age of tide given by low water would be increased.

It is worthy of remark that at Ballycastle the effects depending on the position of the sun and moon *appear* to precede their cause.

Section IX.—*Formation of the time of diurnal high water; progress of the diurnal tide-wave round the island; comparison of its progress and range with those of the semidiurnal tide.*

In page 20 I have given a table of the maximum values of diurnal tide, at high water and at low water. The diurnal tide being supposed to follow the law of sines, its maximum coefficient will be found by taking the square root of the sum of the squares of those two values, and the time after semidiurnal high water at which diurnal high water occurs will be found by taking the angle whose tangent $= \frac{\text{value at low water}}{\text{value at high water}}$, and converting that angle into time at the rate of 360° for a lunar day. The maximum diurnal tide for semidiurnal high water and that for semidiurnal low water may be conceived to hold for any day near to the day of absolute maximum. Thus the following Table is formed.

Station.	July 9.		July 23.		August 6.	
	Coefficient of diurnal tide.	Interval from semidiurnal high water, first division, to diurnal high water.	Coefficient of diurnal tide.	Interval from semidiurnal high water, first division, to diurnal high water.	Coefficient of diurnal tide.	Interval from semidiurnal high water, first division, to diurnal high water.
	ft.	h m	ft.	h m	ft.	h m
Kilbaha	0·55	20 44	0·71	9 5	0·58	20 36
Kilrush	0·56	20 4	0·57	9 0	0·53	20 25
Foynes Island ..	0·81	20 9	0·69	8 14	0·67	20 31
Limerick.....	1·01	19 50	0·69	9 11	0·64	20 50
Casleh Bay.....	0·69	20 12	0·66	8 44	0·63	20 28
Galway	0·81	19 49	0·71	8 37	0·43	20 55
Old Head	0·83	20 56	0·85	9 18	0·98	21 18
Mullaghmore....	0·89	23 25	0·82	11 34	0·86	24 18
Buncrana	0·64	0 19	0·76	13 4	0·70	1 26
Port Rush	0·52	1 5	0·63	14 2	0·69	2 4
Carrowkeel.....	0·47	1 28	0·95	13 1	0·68	1 58
Ballycastle	0·50	0 24	0·68	13 11	0·60	1 4
Glenarm.....	0·96	22 8	0·79	10 3	0·99	22 19
Donaghadee	0·91	21 44	0·85	10 16	1·03	21 40
Ardglass.....	0·82	21 34	0·84	10 23	0·91	21 55
Clogher Head....	0·85	22 2	0·79	10 21	0·83	21 42
Kingstown.....	0·76	21 38	0·67	10 3	0·83	21 58
Dunmore East ..	0·21	2 37	0·33	15 10	0·27	1 38
New Ross	0·22	0 51	0·40	14 33	0·25	0 47
Passage West....	0·15	4 2	0·22	14 13	0·17	1 20
Castle Townsend..	0·16	18 3	0·09	12 41	0·10	20 52

A glance at this table will show how different are the velocities of the diurnal tide-wave and the semidiurnal tide-wave. From Kilbaha to Port Rush, the diurnal tide travels in a direction so different, or with a velocity so small, that it loses $5\frac{1}{2}$ hours in time upon the semidiurnal wave. But it passes through the North Channel with such speed that at Donaghadee it has regained about $4\frac{1}{2}$ hours. Its course however will be better understood by forming the actual time of high diurnal tide, or rather its interval after the moon's transit. I have treated the numbers in the following manner:—Increasing the numbers for July 23rd by $12^h\ 24^m$, or half a tidal day (because the moon's declination then was in a direction opposite to that on July 9th and August 6th), I have three comparable intervals from semidiurnal high water to diurnal high water. I take the mean of these, and apply it to the mean interval from moon's transit to semidiurnal high water in the table of page 39. I also take the mean of the three coefficients. Thus the following Table is formed; in which it is to be remembered that the coefficients are to be taken positive for July 9th and August 6th, and negative for July 23rd.

Station.	Coefficient of diurnal tide.	Interval from moon's transit to diurnal high water.	Station.	Coefficient of diurnal tide.	Interval from moon's transit to diurnal high water.
	ft.	h m		ft.	h m
Kilbaha	0·61	0 51	Ballycastle	0·59	8 25
Kilrush	0·55	0 45	Glenarm.....	0·91	8 15
Foynes Island....	0·72	1 22	Donaghadee	0·93	8 14
Limerick.....	0·78	2 31	Ardglass.....	0·86	8 18
Casleh Bay.....	0·66	0 38	Clogher Head....	0·82	8 36
Galway	0·65	0 40	Kingstown	0·75	8 30
Old Head	0·89	1 35	Dunmore East ..	0·27	7 33
Mullaghmore....	0·86	4 39	New Ross	0·29	7 30
Buncrana	0·70	6 56	Passage West....	0·18	7 47
Port Rush	0·61	8 16	Castle Townsend..	0·12	1 25
Carrowkeel.....	0·70	8 55			

Comparing the coefficients in this table with the column of mean range of semidiurnal tide in page 35, we can discover no analogy between them. The range of diurnal tide is not at all reduced in the North Channel, where the semidiurnal tide is so much diminished; nor (as will also be shown hereafter) is it particularly diminished between Kingstown and Dunmore East, where the semidiurnal tide is nearly or quite obliterated; but it is much diminished at Castle Townsend, where the semidiurnal tide is pretty large.

On examining the interval from moon's transit, it appears evident that the diurnal tide comes from the south, or very nearly from the south. It appears also that it does not pass in either direction through the North Channel, but that the strait is filled simultaneously, or nearly so, at both ends. It appears also that the wave travels very quickly from south to north in the Irish Channel; so quickly indeed that it is probable that the tide is simultaneous throughout. But between Castle Townsend and Passage West it loses more than six hours, or a quarter of a diurnal tide. I am totally unable to explain this. The case is very greatly different from that discussed in page 40, where the change of phase was almost exactly half a tide. I must leave the solution of this difficulty to some more advanced theory of waves.

I may appropriately close this section with a statement, in the form commonly used by nautical persons, of the most prominent effects of the diurnal tide at the several stations.

Assuming that the maximum diurnal tide, with positive sign for the semidiurnal high waters 1st division, occurred about July 9 and August 6, and with opposite sign on July 23, it appears that the maximum takes place when the moon's right ascension is about 9^h. This is not very accurate; first, because the solar diurnal tide is neglected; secondly, because the days adopted are not purely for maximum at semidiurnal high water, but partly also refer to low water. Using however 9^h, it appears from the table in page 39, that the semidiurnal high water at Kilbaha follows the moon's Greenwich transit by $4^h\ 47^m$; and, therefore, when the diurnal tide is greatest, the semidiurnal tide at Kilbaha occurs at $13^h\ 47^m$ Greenwich sidereal time, or $13^h\ 7^m$ Kilbaha sidereal time. If this happens at noon, the sun's right ascension

must be $13^h\ 7^m$, or the day must be about October 12; if it happens at six in the morning, the sun's right ascension must be $19^h\ 7^m$, and the day must be about January 6. In the same way the day may be found for other hours. The coefficient may be taken from the table in page 28, doubling the mean of the quantities in the three high water columns (without regard of sign) for the difference of two tides. Thus the following Table is formed.

Station.	Greatest difference of two high waters on same day.	Day when the excess of noon tide over mid-night tide is greatest.	Day when the excess of morning tide over evening tide is greatest.	Station.	Greatest difference of two high waters on same day.	Day when the excess of noon tide over mid-night tide is greatest.	Day when the excess of morning tide over evening tide is greatest.
	ft.				ft.		
Kilbaha	0·71	Oct. 12.	Jan. 6.	Ballycastle	1·16	Nov. 24.	Feb. 16.
Kilrush	0·55	Oct. 14.	Jan. 8.	Glenarm	1·48	Jan. 7.	April 10.
Foynes Island ..	0·72	Oct. 30.	Jan. 22.	Donaghadee	1·41	Jan. 13.	April 17.
Limerick.....	0·77	Nov. 7.	Jan. 30.	Ardglass.....	1·32	Jan. 13.	April 17.
Casleh Bay.....	0·65	Oct. 14.	Jan. 8.	Clogher Head....	1·29	Jan. 16.	April 20.
Galway	0·61	Oct. 17.	Jan. 10.	Kingstown	1·16	Jan. 19.	April 23.
Old Head	1·14	Oct. 17.	Jan. 10.	Dunmore East ..	0·44	Oct. 21.	Jan. 5.
Mullaghmore	1·65	Oct. 26.	Jan. 18.	New Ross	0·53	Nov. 2.	Jan. 24.
Buncrana	1·37	Nov. 5.	Jan. 28.	Passage West....	0·30	Oct. 21.	Jan. 14.
Port Rush	1·11	Nov. 14.	Feb. 3.	Castle Townsend..	0·08	Oct. 13.	Jan. 7.
Carrowkeel.....	1·31	Nov. 26.	Feb. 18.				

Section X.—*Method of expressing the height of the water, throughout every individual tide, by sines and cosines of arcs, and expressions in this form for every tide in the whole series of observations, except those at Courtown.*

The times of high water (and similarly those of low water) having had their principal irregularities smoothed down by the operations described in Section II., and being corrected for the diurnal equation in time ascertained by the operations of Section III., present a series of times, which are liable perhaps to something like constant error from the method involuntarily adopted by the computer in fixing on the time of high water, and which are affected by the peculiar form of the tidal function at each station, but which nevertheless follow at intervals equal (with very considerable accuracy) to the true tidal day of the place. This being understood, it will be seen that the following process entirely corrects any error of the supposed times of high or low water in its exhibition of the time of maximum of the first tidal argument, and is entirely free from the effects of such error in the exhibition of other quantities.

The whole number of observations, equidistant in time, made in the course of one tide, being about 150, if we divide this duration into sixteen equal parts we shall have at least nine observations in each part; and the mean time of these nine observations cannot in any case be more than $2\frac{1}{2}$ minutes from the middle of that part. It appears evident here that we may use the mean of all the heights in one portion to represent (with smaller error than unavoidably occurs in the observations) the

mean which would have been obtained if observations had been taken at infinitely small equal intervals. The same remark applies in a stronger degree if the whole duration be divided into twelve parts.

Let us use the term *phase* for an angle proportional to the time which increases by 360° in a complete tide; and let it be assumed that the height of the water can be expressed by the following formula:

$$A_0+A_1 \sin \text{phase} +A_2 \sin 2 \text{ phase} +A_3 \sin 3 \text{ phase} +A_4 \sin 4 \text{ phase}$$
$$+B_1 \cos \text{phase} +B_2 \cos 2 \text{ phase} +B_3 \cos 3 \text{ phase} +B_4 \cos 4 \text{ phase},$$

and suppose that the complete tide, or 360° of phase, is divided into sixteen equal parts and the mean height in each part taken.

The mean height in the first part will be,

$$A_0+\frac{8}{\pi}A_1\left(\cos 0-\cos\frac{\pi}{8}\right)+\frac{8}{2\pi}A_2\left(\cos 0-\cos\frac{2\pi}{8}\right)+\frac{8}{3\pi}A_3\left(\cos 0-\cos\frac{3\pi}{8}\right)+\frac{8}{4\pi}A_4\left(\cos 0-\cos\frac{4\pi}{8}\right)$$
$$+\frac{8}{\pi}B_1\left(\sin\frac{\pi}{8}-\sin 0\right)+\frac{8}{2\pi}B_2\left(\sin\frac{2\pi}{8}-\sin 0\right)+\frac{8}{3\pi}B_3\left(\sin\frac{3\pi}{8}-\sin 0\right)+\frac{8}{4\pi}B_4\left(\sin\frac{4\pi}{8}-\sin 0\right).$$

The mean height in the second part will be,

$$A_0+\frac{8}{\pi}A_1\left(\cos\frac{\pi}{8}-\cos\frac{2\pi}{8}\right)+\frac{8}{2\pi}A_2\left(\cos\frac{2\pi}{8}-\cos\frac{4\pi}{8}\right)+\frac{8}{3\pi}A_3\left(\cos\frac{3\pi}{8}-\cos\frac{6\pi}{8}\right)+\frac{8}{4\pi}A_4\left(\cos\frac{4\pi}{8}-\cos\frac{8\pi}{8}\right)$$
$$+\frac{8}{\pi}B_1\left(\sin\frac{2\pi}{8}-\sin\frac{\pi}{8}\right)+\frac{8}{2\pi}B_2\left(\sin\frac{4\pi}{8}-\sin\frac{2\pi}{8}\right)+\frac{8}{3\pi}B_3\left(\sin\frac{6\pi}{8}-\sin\frac{3\pi}{8}\right)+\frac{8}{4\pi}B_4\left(\sin\frac{8\pi}{8}-\sin\frac{4\pi}{8}\right),$$

and so on.

Now if we group these in the following manner,

$$(1\text{st}+5\text{th}+9\text{th}+13\text{th})+(2\text{nd}+6\text{th}+10\text{th}+14\text{th})-(3\text{rd}+7\text{th}+11\text{th}+15\text{th})$$
$$-(4\text{th}+8\text{th}+12\text{th}+16\text{th}),$$

the sum will be $\frac{32}{\pi}A_4$.

If we group them in the following manner,

$$(1\text{st}+5\text{th}+9\text{th}+13\text{th})-(2\text{nd}+6\text{th}+10\text{th}+14\text{th})-(3\text{rd}+7\text{th}+11\text{th}+15\text{th})$$
$$+(4\text{th}+8\text{th}+12\text{th}+16\text{th}),$$

the sum will be $\frac{32}{\pi}B_4$.

If we unite the adjacent means and group them thus,

$$\overline{(1\text{st}+2\text{nd}+9\text{th}+10\text{th})}+\overline{(3\text{rd}+4\text{th}+11\text{th}+12\text{th})}-\overline{(5\text{th}+6\text{th}+13\text{th}+14\text{th})}$$
$$-\overline{(7\text{th}+8\text{th}+15\text{th}+16\text{th})},$$

the sum will be $\frac{32}{\pi}A_2$.

If we group them in this manner,

$$\overline{(1\text{st}+2\text{nd}+9\text{th}+10\text{th})}-\overline{(3\text{rd}+4\text{th}+11\text{th}+12\text{th})}-\overline{(5\text{th}+6\text{th}+13\text{th}+14\text{th})}$$
$$+\overline{(7\text{th}+8\text{th}+15\text{th}+16\text{th})},$$

the sum will be $\frac{32}{\pi}B_2$.

If we unite the adjacent numbers already formed by union, so as to have the sum of four adjacent means together, and combine them thus,

(1st+2nd+3rd+4th)+(5th+6th+7th+8th)−(9th+10th+11th+12th)
−(13th+14th+15th+16th),

the sum is $\frac{32}{\pi}A_1+\frac{32}{3\pi}A_3$.

If we combine them in this manner,

(1st+2nd+3rd+4th)−(5th+6th+7th+8th)−(9th+10th+11th+12th)
+(13th+14th+15th+16th),

the sum is $\frac{32}{\pi}B_1-\frac{32}{3\pi}B_3$.

Then if we divide the complete tide into twelve equal parts, and take the mean height in each, we shall have

Mean height in the first part =

$$A_0+\frac{6}{\pi}A_1\left(\cos 0-\cos\frac{\pi}{6}\right)+\frac{6}{2\pi}A_2\left(\cos 0-\cos\frac{2\pi}{6}\right)+\frac{6}{3\pi}A_3\left(\cos 0-\cos\frac{3\pi}{6}\right)+\frac{6}{4\pi}A_4\left(\cos 0-\cos\frac{4\pi}{6}\right)$$
$$+\frac{6}{\pi}B_1\left(\sin\frac{\pi}{6}-\sin 0\right)+\frac{6}{2\pi}B_2\left(\sin\frac{2\pi}{6}-\sin 0\right)+\frac{6}{3\pi}B_3\left(\sin\frac{3\pi}{6}-\sin 0\right)+\frac{6}{4\pi}B_4\left(\sin\frac{4\pi}{6}-\sin 0\right),$$

and so on. And combining these in the following manner,

(1st+5th+9th)+(2nd+6th+10th)−(3rd+7th+11th)−(4th+8th+12th),

the sum will be $\frac{24}{\pi}A_3$, or the sum $\times\frac{4}{3}=\frac{32}{\pi}A_3$.

And if we combine them in the following manner,

(1st+5th+9th)−(2nd+6th+10th)−(3rd+7th+11th)+(4th+8th+12th),

the sum will be $\frac{24}{\pi}B_3$, or the sum $\times\frac{4}{3}=\frac{32}{\pi}B_3$.

By applying one-third of these to the expressions last found, we shall obtain $\frac{32}{\pi}A_1$ and $\frac{32}{\pi}B_1$.

The mean of all the means, either in the division by sixteen or in that by twelve, is A_0.

The whole of these operations (after taking the means of the original observations) are performed with great facility, and without the possibility of mistake, by means of a printed skeleton form, of which a specimen will be given shortly.

The next thing to be considered is, how we shall correct these numbers for the effect of diurnal tide, which is included in the observations, but from which our formula for semidiurnal tide is to be freed. Suppose that the tide begins with high water, and suppose a to be the effect of diurnal tide at that high water, b the effect of diurnal tide at the low water following, or that which occurs in the middle of the tide. Then the complete effect of diurnal tide is represented by

$$a\,.\cos\frac{\text{phase}}{2}+b\,.\sin\frac{\text{phase}}{2};$$

and the question now is, how this function can be represented, through the course of one tide, by a formula similar to

$$A_0 + A_1 \sin \text{phase} + A_2 \sin 2 \text{ phase} + \&c.$$
$$+ B_1 \cos \text{phase} + B_2 \cos 2 \text{ phase} + \&c.$$

For this purpose I have taken the mean value of the function for each sixteenth part, and for each twelfth part, of the entire circle of phase, and have combined these numbers according to the rules just laid down. The result is that

The sum $\frac{32}{\pi} A_4$ is increased by $\frac{16}{\pi} \times 0{\cdot}3980 \times a$.

The sum $\frac{32}{\pi} B_4$ is increased by $-\frac{16}{\pi} \times 0{\cdot}0392 \times b$.

The sum $\frac{32}{\pi} A_2$ is increased by $\frac{16}{\pi} \times 0{\cdot}8284 \times a$.

The sum $\frac{32}{\pi} B_2$ is increased by $-\frac{16}{\pi} \times 0{\cdot}1648 \times b$.

The sum $\frac{32}{\pi} A_1 + \frac{32}{3\pi} A_3$ is increased by $\frac{16}{\pi} \times 2a$.

The sum $\frac{32}{\pi} B_1 - \frac{32}{3\pi} B_3$ is increased by $-\frac{16}{\pi} \times 0{\cdot}8284 \times b$.

The sum $\frac{24}{\pi} A_3$ is increased by $\frac{12}{\pi} \times 0{\cdot}5360 \times a$.

The sum $\frac{24}{\pi} B_3$ is increased by $-\frac{12}{\pi} \times 0{\cdot}0704 \times b$.

The mean, or A_0, is increased by $+\frac{2}{\pi} b$.

The corrections are to be applied with opposite signs, in order to free from the effects of diurnal tide the results given by the observations.

Another cause for which a correction is due is, the difference of height at the beginning and at the end (the correction for diurnal tide being previously applied). If the whole rise c be supposed to have come by uniform degrees, the effects produced in the sums $\frac{32}{\pi} A_4$, $\frac{32}{\pi} A_2$, $\frac{32}{\pi} A_1 + \frac{32}{3\pi} A_3$, and $\frac{24}{\pi} A_3$, are $-c$, $-2c$, $-4c$, and $-c$. The corrections must have opposite signs.

In this manner (confining ourselves for a moment to the consideration of 4 phase) we have such expressions as $\frac{32}{\pi} A_4$ and $\frac{32}{\pi} B_4$. And the quantity which we wish to obtain is $A_4 \sin 4 \text{ phase} + B_4 \cos 4 \text{ phase}$, which may be converted into one of this form, $\sqrt{A_4^2 + B_4^2} \times \sin . \overline{4 \text{ phase} + \varphi}$, where $\tan \varphi = \frac{B_4}{A_4}$. The coefficient is

$$= \frac{\pi}{32} \sqrt{\left(\frac{32}{\pi} A_4\right)^2 + \left(\frac{32}{\pi} B_4\right)^2}.$$

As, in the analysis of all the tides, this transformation was to be performed about 6000 times, it was highly important to devise an easy method of effecting it. For this purpose the following mechanical arrangement was contrived. Upon a nearly square piece of pasteboard were carefully traced two scales at right angles to each other, with graduations of equal parts proceeding from the point of union. Upon the edge of another narrow piece of pasteboard was traced a graduation whose parts were to the parts of the former in the proportion of 3·2 to 3·1416. The commencing point of this graduation was made the centre of a quadrant, of which one radius was in the line of graduation produced. The divisions of the quadrant, proceeding from the line of graduation, were marked from 0 to 90°, and also from 360° to 270°. The method of using it was; to insert a needle at the centre of the quadrant, and to plant its point upon one of the lines of the large pasteboard at the graduation corresponding to $\frac{32}{\pi}A_4$; then to plant a second needle in the other line of the large pasteboard at the graduation corresponding to $\frac{32}{\pi}B_4$, and to turn the graduated edge of the long piece till it touched this second needle; the reading of the graduated edge, with a shift of the decimal point, gave $\frac{\pi}{32}\sqrt{\left\{\left(\frac{32}{\pi}A_4\right)^2+\left(\frac{32}{\pi}B_4\right)^2\right\}}$ or $\sqrt{(A_4^2+B_4^2)}$; and the division of the quadrant cut by the straight line on the large pasteboard gave φ. When the signs of $\frac{32}{\pi}A_4$ and $\frac{32}{\pi}B_4$ are the same, the reading between 0 and 90° is to be taken; when different, that between 360° and 270° is to be taken; and in either case, when $\frac{32}{\pi}A_4$ is negative, 180° is to be added or subtracted.

Applying the same process to the four combinations of A_1 and B_1, A_2 and B_2, A_3 and B_3, A_4 and B_4, we have the height of the water at every instant expressed by the formula

$$A_0+C_1\sin(\text{phase}+\varphi_1)+C_2\sin(2\text{ phase}+\varphi_2)+C_3\sin(3\text{ phase}+\varphi_3)+C_4\sin(4\text{ phase}+\varphi_4),$$

where the *phase* is an angle which is measured from the assumed commencement of the tide, and may be converted into time by multiplying it by $\frac{\text{whole duration of tide}}{360°}$. It is evident however that the argument (phase $+\varphi_1$) commences at the time when the water would be at its mean height before attaining its greatest height, if the oscillation of surface were supposed to depend on that term only. The time of high water, on the same supposition, would be given by making phase $+\varphi_1=90°$, or phase $=90°-\varphi_1$; converting the expression, when found, into time by the rule above. Or the time of low water, on the same supposition, would be given by making phase $+\varphi_1=270°$, or phase $=270°-\varphi_1$. It is convenient to choose, of these two expressions, that which gives the smaller quantity. The quantity so found is to be added to the Greenwich time of assumed commencement of tide, and it gives the Greenwich time of high

water or low water, on the supposition that the fluctuation depends entirely on the argument (phase $+\varphi_1$). Since the Theory of Waves, as applied to Tides, leads us to refer every angle to that argument, and induces us to suppose that the term connected with that argument is the only one which is immediately created by the tidal forces (the others depending numerically on it almost in the same way in which the coefficients of successive multiples of *anomaly* depend on that of the simple *anomaly*), it appears to be proper to consider the times of high and low water thus found as the genuine times of high and low water. For the sake of distinction I call them the Analysed Times.

As it is convenient to use the time of one phase only, I have, when the analysis gave the Analysed Times of two low waters, taken their mean for the Analysed Time of High Water.

Now if we put phase $+\varphi_1=p$, $\varphi_2-2\varphi_1=c_2$, $\varphi_3-3\varphi_1=c_3$, $\varphi_4-4\varphi_1=c_4$, our expression for the height at every instant will be

$$A_0+C_1 \sin p+C_2 \sin (2p+c_2)+C_3 \sin (3p+c_3)+C_4 \sin (4p+c_4),$$

and this, with a statement of the time at which the argument p has the value of 90° (or the Analysed Time of High Water), gives a complete knowledge of the form of every tide.

I annex a specimen of the printed skeleton form in which the calculations described in this section were made (the figures, and the words in italics, being inserted for each special tide). And I subjoin the whole of the results for the twenty-one stations. Each line is the digested result of about 170 observations.

Station—*Limerick*. Limits of Tide, *June* 25d 20h 38m to *June* 26d 8h 52m. Commencing with corrected *High* Water. Coefficients for Diurnal Tide, $a = -0^{f}{\cdot}30$, $b = +1^{f}{\cdot}13$.
Rise of Tide from beginning to end (after correction for Diurnal Tide) $= 0^{f}{\cdot}88$.

Means of Heights in Sixteenth Portions of Tide. For 4 Phase.					Sums of adjacent Means. For 2 Phase.				Sums of adjacent Sums. For Phase.				Means of Heights in Twelfth Portions of Tides. For 3 Phase.				
1st portion	18·33	.....	.....	.....	35·61	.....	.....	.....	63·54	.....	.....	.....	1st	18·20	.....	.....	.....
2nd portion	.....	17·28	.....	.....									2nd	.....	16·17	.....	.....
3rd portion	.....	.....	15·10	.....	.....	27·93	.....	.....					3rd	.....	.....	13·19	.....
4th portion	.....	.....	.....	12·83									4th	.....	.....	.....	9·95
5th portion	10·39	.....	.....	.....	.....	.....	18·12	.....	.....	26·68	.....	.....	5th	6·33	.....	.....	.....
6th portion	.....	7·73	.....	.....									6th	.....	3·72	.....	.....
7th portion	.....	.....	5·07	.....	.....	.....	.....	8·56					7th	.....	.....	3·09	.....
8th portion	.....	.....	.....	3·49									8th	.....	.....	.....	7·56
9th portion	2·71	.....	.....	.....	8·38	.....	.....	.....	.....	.....	29·67	.....	9th	11·60	.....	.....	.....
10th portion	.....	5·67	.....	.....									10th	.....	14·16	.....	.....
11th portion	.....	.....	9·27	.....	.....	21·29	.....	.....					11th	.....	.....	16·64	.....
12th portion	.....	.....	.....	12·02									12th	.....	.....	.....	18·02
13th portion	13·82	.....	.....	.....	.....	.....	29·50	.....	.....	.....	.....	64·97					
14th portion	.....	15·68	.....	.....													
15th portion	.....	.....	17·36	.....	.....	.....	.....	35·47									
16th portion	.....	.....	.....	18·11													
Sums	45·25 I.	46·36 II.	46·80 III.	46·45 IV.	43·99 I.	49·22 II.	47·62 III.	44·03 IV.	63·54 I.	26·68 II.	29·67 III.	64·97 IV.		36·13 I.	34·05 II.	32·92 III.	35·53 IV.
I. + II.				91·61				93·21				90·22					70·18
Subtract III. + IV.				93·25				91·65				94·64					68·45
				− 1·64				+ 1·56				− 4·42					+ 1·73
Add for a				+ 0·61				+ 1·27				+ 2·79					+ 0·61
				− 1·03				+ 2·83				− 1·63					+ 2·34
Add for Rise				− 0·88				− 1·76				− 3·23					− 0·88
												− 4·86					+ 1·46
									Add − ⅓ No. for sine 3 phase			− 0·65				Add one-third	+ 0·49
	Number for sine 4 phase			− 1·91	Number for sine 2 phase			+ 1·07	Number for sine phase			− 5·51		Number for sine 3 phase			+ 1·95
I. + IV.				91·70				88·02				128·51					71·66
Subtract II. + III.				93·16				96·84				56·35					66·97
				− 1·46				− 8·82				+72·16					+ 4·69
Add for b				+ 0·23				+ 0·95				+ 4·90					+ 0·31
												+77·06					+ 5·00
									Add ⅓ No. for cosine 3 phase			+ 2·22				Add one-third	+ 1·67
	Number for cosine 4 phase			− 1·23	Number for cosine 2 phase			− 7·87	Number for cosine phase			+79·28		Number for cosine 3 phase			+ 6·67
	I. + II. + III. + IV. = 184·86				One-fourth = 46·21 Add for b				One-sixteenth = 11·55 − 0·72					I. + II. + III. + IV. = 138·63 one-twelfth = 11·55 Add for b − 0·72			

Therefore the expression for the height in semidiurnal tide is

$$\left\{\begin{array}{l}\text{ft.}\\ 10{\cdot}83\\ +7{\cdot}82\times \text{sine}(\text{phase} + 93^\circ\ 45')\\ +0{\cdot}78\times \text{sine}(2\,\text{phase} + 277^\circ\ 35')\\ +0{\cdot}68\times \text{sine}(3\,\text{phase} + 73^\circ\ 45')\\ +0{\cdot}22\times \text{sine}(4\,\text{phase} + 213^\circ\ 35')\end{array}\right\} \text{or} \left\{\begin{array}{l}\text{ft.}\\ 10{\cdot}83\\ +7{\cdot}82\times \text{sine}\ p\\ +0{\cdot}78\times \text{sine}(2p + 90^\circ\ 5')\\ +0{\cdot}68\times \text{sine}(3p + 152^\circ\ 30')\\ +0{\cdot}22\times \text{sine}(4p + 198^\circ\ 35').\end{array}\right\}$$

And the correction to the assumed times of *High* Water is $\frac{-3^\circ\ 45' \times 735^m}{360^\circ} = -8^m$.

(Note. The first term is yet to be increased by $7^{f}{\cdot}58$, for the difference of level between the zero of the tide-gauge and the point 30 feet below the bolt at Buckingham Lock.)

Height of the Water in each individual tide at Kilbaha, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
June 24. 18 29 / 25. 6 39	16·40	5·10	0·14	146	0·19	215	0·06	115
25. 18 59 / 26. 7 13	15·79	5·05	0·13	199	0·09	318	0·01	40
26. 19 27 / 27. 7 46	15·50	4·71	0·18	170	0·03	291	0·05	140
27. 20 0 / 28. 8 20	15·65	4·20	0·08	158	0·07	204	0·09	82
28. 20 43 / 29. 9 0	15·52	3·76	0·13	199	0·05	192	0·02	99
29. 21 32 / 30. 10 2	15·65	3·25	0·10	210	0·08	244	0·02	55
30. 22 25	15·70	2·93	0·04	144	0·06	227	0·03	151
July 1. 10 57								
1. 23 29	15·51	2·73	0·06	216	0·08	231	0·02	52
2. 12 2								
3. 0 35	15·96	2·82	0·02	232	0·07	225	0·06	109
3. 13 11								
4. 1 46	16·41	3·36	0·12	212	0·07	281	0·04	55
4. 14 13								
5. 2 39	15·49	3·95	0·12	216	0·09	262	0·03	37
5. 15 6								
6. 3 32	15·43	4·73	0·10	187	0·09	288	0·04	230
6. 15 54 / 7. 4 21	16·00	5·32	0·04	153	0·01	210	0·01	289
7. 16 42 / 8. 5 0	16·05	5·86	0·03	225	0·09	255	0·04	88
8. 17 22 / 9. 5 42	15·96	6·33	0·14	132	0·04	217	0·02	44
9. 18 16 / 10. 6 40	15·98	6·80	0·17	142	0·14	248	0·08	61
10. 18 46 / 11. 7 10	16·12	6·80	0·26	155	0·14	305	0·07	154
11. 19 39 / 12. 8 2	15·79	6·48	0·20	175	0·02	121	0·07	140
12. 20 31 / 13. 8 55	15·46	5·88	0·29	211	0·10	226	0·03	67
13. 21 21 / 14. 9 48	15·26	4·97	0·17	200	0·09	220	0·02	122
14. 22 24	15·39	4·36	0·08	205	0·11	256	0·01	176
15. 10 58								
15. 23 32	16·01	3·72	0·19	196	0·06	328	0·08	228
16. 12 10								
17. 0 48	16·00	3·47	0·09	237	0·04	247	0·04	4
17. 13 27								
18. 2 5	15·80	3·70	0·15	238	0·11	265	0·04	207
18. 14 36								
19. 3 6	15·72	4·15	0·09	276	0·05	288	0·03	148
19. 15 32 / 20. 3 59	15·76	4·48	0·03	152	0·07	215	0·05	210
20. 16 20 / 21. 4 38	15·54	4·93	0·11	161	0·00	219	0·04	137
21. 16 53 / 22. 5 9	15·56	5·35	0·08	182	0·02	350	0·04	174
22. 17 28 / 23. 5 45	15·51	5·50	0·13	166	0·01	112	0·03	28

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 18 2	15·47	5·55	0·14	152	0·05	226	0·02	28
24. 6 18								
24. 18 32	15·58	5·47	0·20	140	0·03	128	0·03	56
25. 6 52								
25. 19 5	15·48	5·19	0·18	154	0·01	214	0·03	27
26. 7 22								
26. 19 39	15·31	4·81	0·16	175	0·02	209	0·05	77
27. 7 55								
27. 20 12	15·28	4·28	0·12	187	0·07	196	0·04	236
28. 8 27								
28. 20 52	15·17	3·69	0·15	189	0·06	226	0·02	47
29. 9 11								
29. 21 32	15·32	3·30	0·15	149	0·09	267	0·02	66
30. 9 59								
30. 22 26	15·25	2·73	0·04	204	0·05	232	0·03	158
31. 11 6								
31. 23 46	15·45	2·57	0·06	182	0·05	199	0·02	338
August 1. 12 27								
2. 1 8	15·75	2·90	0·03	218	0·03	76	0·02	132
2. 13 43								
3. 2 18	15·38	3·62	0·01	261	0·05	240	0·06	60
3. 14 45								
4. 3 11	15·45	4·55	0·07	205	0·09	215	0·11	104
4. 15 34	15·68	5·30	0·03	192	0·10	218	0·04	197
5. 3 54								
5. 16 18	15·70	6·32	0·05	135	0·08	233	0·05	0
6. 4 42								
6. 17 2	15·80	7·30	0·21	170	0·04	7	0·04	184
7. 5 22								
7. 17 39	15·89	7·63	0·18	148	0·05	127	0·07	100
8. 6 2								
8. 18 27	15·94	7·62	0·32	168	0·03	266	0·05	91
9. 6 48								
9. 19 10	15·76	7·08	0·24	161	0·10	221	0·07	74
10. 7 34								
10. 19 55	15·63	6·12	0·29	168	0·06	193	0·05	71
11. 8 21								
11. 20 45	15·64	5·01	0·19	178	0·07	206	0·06	72
12. 9 8								
12. 21 45	15·41	4·06	0·13	227	0·13	242	0·04	159
13. 10 22								
13. 22 58	15·27	3·24	0·10	244	0·11	309	0·02	180
14. 11 43								
15. 0 27	15·27	2·79	0·12	221	0·07	269	0·01	0
15. 13 9								
16. 1 50	15·41	3·16	0·06	235	0·10	268	0·02	316
16. 14 19								
17. 2 48	15·59	3·71	0·05	238	0·05	279	0·00	51
17. 15 10								
18. 3 31	15·64	4·32	0·09	203	0·05	270	0·01	24
18. 15 50	15·76	4·76	0·08	121	0·05	178	0·02	108
19. 4 11								
19. 16 29	15·74	5·29	0·13	165	0·09	242	0·03	189
20. 4 43								
20. 16 55	15·54	5·73	0·11	177	0·06	232	0·04	163
21. 5 11								
21. 17 26	15·24	5·86	0·16	149	0·05	154	0·04	127
22. 5 38								
22. 17 54	15·58	5·80	0·15	135	0·10	222	0·04	88
23. 6 12								

Height of the Water in each individual tide at Kilrush, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
June 23. 17 50	17·14	5·90	0·17	111	0·07	284	0·07	167
24. 6 11								
24. 18 42	17·20	5·53	0·21	105	0·08	155	0·06	141
25. 6 52								
25. 19 14	16·62	5·47	0·12	119	0·05	261	0·11	175
26. 7 28								
26. 19 32	16·29	5·18	0·20	124	0·07	231	0·10	146
27. 7 51								
27. 20 23	16·40	4·63	0·16	150	0·05	153	0·04	107
28. 8 43								
28. 20 52	16·48	4·23	0·42	116	0·17	139	0·03	97
29. 9 17								
29. 21 41	16·36	3·80	0·16	106	0·05	194	0·05	130
30. 10 7								
30. 22 33	16·52	3·39	0·11	86	0·01	30	0·03	230
July 1. 11 4								
1. 23 35	16·30	3·12	0·10	135	0·09	186	0·00	130
2. 12 13								
3. 0 50	16·74	3·22	0·10	84	0·06	208	0·01	240
3. 13 21								
4. 1 52	17·25	3·75	0·16	150	0·07	201	0·02	269
4. 14 19								
5. 2 45	16·51	4·50	0·12	136	0·09	237	0·01	333
5. 14 58	16·20	5·13	0·15	314	0·17	177	0·08	23
6. 3 26								
6. 15 57	16·75	5·71	0·17	58	0·06	257	0·02	181
7. 4 24								
7. 16 52	16·90	6·30	0·11	80	0·11	258	0·06	134
8. 5 10								
8. 17 25	16·63	6·87	0·20	77	0·12	155	0·05	205
9. 5 45								
9. 18 9	16·86	7·17	0·31	99	0·09	254	0·13	184
10. 6 33								
10. 18 58	17·02	7·15	0·23	103	0·02	239	0·05	210
11. 7 22								
11. 19 40	16·70	6·82	0·17	100	0·04	76	0·05	119
12. 8 3								
12. 20 32	16·35	6·13	0·24	152	0·09	161	0·01	44
13. 8 56								
13. 21 21	16·00	5·43	0·18	125	0·06	194	0·03	179
14. 9 48								
14. 22 25	16·10	4·78	0·10	159	0·13	264	0·05	170
15. 10 58								
15. 23 30	16·63	4·18	0·18	151	0·07	267	0·02	318
16. 12 11								
17. 0 51	16·70	3·99	0·07	190	0·03	201	0·02	218
17. 13 28								
18. 2 4	16·60	4·11	0·11	140	0·09	241	0·04	201
18. 14 36								
19. 3 8	16·45	4·64	0·05	128	0·03	254	0·07	167
19. 15 30	16·46	5·01	0·10	60	0·08	173	0·05	136
20. 3 57								
20. 16 24	16·24	5·43	0·13	101	0·04	112	0·03	167
21. 4 42								
21. 17 3	16·29	5·82	0·12	100	0·04	194	0·03	144
22. 5 19								
22. 17 39	16·28	6·02	0·20	111	0·04	179	0·03	193
23. 5 56								

The value $c_2=314°$ for July 5 and 6 is correct.

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 18 12	16·31	6·02	0·21	120	0·09	228	0·05	205
24. 6 28								
24. 18 40	16·36	5·87	0·26	100	0·06	184	0·03	123
25. 7 0								
25. 19 15	16·21	5·69	0·26	122	0·04	206	0·07	146
26. 7 32								
26. 19 50	15·98	5·29	0·15	153	0·02	211	0·01	95
27. 8 6								
27. 20 18	15·95	4·82	0·19	129	0·07	165	0·08	146
28. 8 33								
28. 20 56	15·80	4·20	0·20	143	0·06	204	0·02	259
29. 9 15								
29. 21 33	16·02	3·72	0·23	118	0·10	215	0·02	119
30. 10 6								
30. 22 38	15·98	3·10	0·09	142	0·04	224	0·01	253
31. 11 15								
31. 23 51	16·14	2·89	0·12	117	0·09	210	0·01	29
August 1. 12 31								
2. 1 10	16·59	3·33	0·11	143	0·06	226	0·04	206
2. 13 52								
3. 2 33	16·32	4·15	0·16	124	0·08	223	0·02	213
3. 15 7	16·34	4·89	0·13	117	0·06	234	0·03	266
4. 3 35								
4. 15 57	16·64	5·86	0·11	115	0·03	285	0·10	185
5. 4 17								
5. 16 42	16·68	6·87	0·22	82	0·07	195	0·06	151
6. 5 6								
6. 17 30	16·80	7·59	0·25	71	0·11	219	0·06	128
7. 5 50								
7. 18 9	16·76	7·96	0·34	91	0·10	208	0·08	199
8. 6 32								
8. 18 57	16·69	7·95	0·25	116	0·11	217	0·03	231
9. 7 18								
9. 19 38	16·51	7·49	0·24	113	0·10	198	0·02	123
10. 8 2								
10. 20 23	16·36	6·62	0·29	127	0·16	197	0·07	78
11. 8 49								
11. 21 12	16·38	5·50	0·15	138	0·11	183	0·06	116
12. 9 35								
12. 22 7	16·17	4·58	0·11	150	0·11	213	0·07	175
13. 10 41								
13. 23 14	15·98	3·76	0·13	148	0·08	240	0·07	212
14. 11 59								
15. 0 43	16·07	3·29	0·12	153	0·06	235	0·02	284
15. 13 25								
16. 2 7	16·24	3·69	0·10	148	0·05	265	0·03	190
16. 14 37								
17. 3 7	16·53	4·30	0·11	117	0·03	123	0·01	231
17. 15 28	16·39	4·70	0·15	31	0·15	208	0·08	117
18. 3 50								
18. 16 10	16·55	5·33	0·14	84	0·11	170	0·01	209
19. 4 31								
19. 16 54	16·53	5·80	0·24	112	0·06	216	0·07	164
20. 5 8								
20. 17 16	16·31	6·20	0·13	60	0·08	211	0·03	65
21. 5 32								
21. 17 50	16·00	6·32	0·21	109	0·12	150	0·04	130
22. 6 2								
22. 18 13	16·42	6·36	0·18	68	0·14	176	0·08	72
23. 6 31								

Height of the Water in each individual tide at Foynes Island, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
June 25. 19 55	17·57	6·38	0·36	122	0·15	160	0·08	289
26. 8 9								
26. 20 26	17·05	6·01	0·49	116	0·15	166	0·06	232
27. 8 45								
27. 21 0	17·21	5·52	0·42	128	0·11	168	0·04	107
28. 9 20								
28. 21 34	17·08	5·22	0·39	116	0·12	143	0·02	274
29. 9 52								
29. 22 12	17·03	4·57	0·38	107	0·08	160	0·04	71
30. 10 37								
30. 23 3	17·25	4·16	0·28	113	0·06	163	0·04	152
July 1. 11 34								
2. 0 4	17·14	3·91	0·34	122	0·09	146	0·03	268
2. 12 40								
3. 1 16	17·43	3·88	0·39	127	0·16	192	0·04	351
3. 13 55								
4. 2 33	18·04	4·38	0·34	126	0·13	182	0·04	331
4. 14 56	17·37	5·00	0·21	111	0·19	189	0·03	280
5. 3 20								
5. 15 52	16·88	5·81	0·43	115	0·13	185	0·02	167
6. 4 24								
6. 16 54	17·48	6·50	0·41	109	0·19	163	0·03	329
7. 5 21								
7. 17 48	17·67	7·13	0·34	113	0·22	165	0·11	25
8. 6 6								
8. 18 30	17·40	7·68	0·54	115	0·37	165	0·07	279
9. 6 50								
9. 19 20	17·38	8·03	0·50	128	0·28	199	0·15	271
10. 7 44								
10. 20 2	17·70	7·94	0·60	102	0·37	179	0·02	20
11. 8 26								
11. 20 48	17·43	7·66	0·57	116	0·24	167	0·03	4
12. 9 11								
12. 21 38	17·40	7·22	0·51	114	0·30	159	0·06	287
13. 9 59								
13. 22 20	16·77	6·57	0·55	119	0·19	175	0·09	282
14. 10 47								
14. 23 14	16·71	5·65	0·41	128	0·14	189	0·02	30
15. 11 46								
16. 0 18	17·16	4·97	0·36	131	0·12	175	0·07	33
16. 12 57								
17. 1 35	17·35	4·72	0·32	144	0·13	162	0·03	348
17. 14 11								
18. 2 47	17·29	4·91	0·35	137	0·15	186	0·04	310
18. 15 21	17·18	5·25	0·32	125	0·10	183	0·01	290
19. 3 50								
19. 16 15	17·18	5·77	0·44	120	0·14	155	0·03	280
20. 4 42								
20. 17 8	17·00	6·28	0·47	120	0·14	168	0·03	264
21. 5 26								
21. 17 48	17·02	6·65	0·47	116	0·18	179	0·02	284
22. 6 4								
22. 18 22	17·08	6·82	0·59	109	0·29	165	0·05	220
23. 6 39								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 18 55 } 24. 7 11 }	16·99	6·89	0·61	113	0·21	172	0·02	10
24. 19 23 } 25. 7 43 }	17·06	6·80	0·60	115	0·18	159	0·04	306
25. 19 55 } 26. 8 12 }	16·93	6·50	0·61	116	0·22	160	0·02	307
26. 20 25 } 27. 8 41 }	16·77	6·11	0·58	123	0·16	159	0·00	202
27. 20 54	16·78	5·80	0·55	110	0·15	152	0·05	92
28. 9 11								
28. 21 27	16·60	5·19	0·47	110	0·10	139	0·06	135
29. 9 47								
29. 22 6	16·78	4·52	0·46	113	0·09	186	0·01	210
30. 10 33								
30. 23 0	16·68	3·86	0·35	119	0·06	173	0·04	61
31. 11 39								
August 1. 0 17	16·89	3·64	0·39	127	0·13	190	0·03	335
1. 13 1								
2. 1 45	17·20	4·07	0·38	137	0·12	198	0·05	304
2. 14 24								
3. 3 2	17 00	4·89	0·44	131	0·12	189	0·03	269
3. 15 37 } 4. 4 7 }	17·16	5·66	0·34	115	0·13	171	0·00	122
4. 16 37 } 5. 4 57 }	17·38	6·59	0·43	121	0·22	165	0·02	287
5. 17 27 } 6. 5 51 }	17·38	7·71	0·55	107	0·29	160	0·01	35
6. 18 17 } 7. 6 37 }	17·53	8·29	0·64	102	0·37	162	0·04	259
7. 18 57 } 8. 7 20 }	17·57	8·58	0·74	112	0·45	165	0·11	249
8. 19 45 } 9. 8 6 }	17·56	8·51	0·72	114	0·39	171	0·09	283
9. 20 25 } 10. 8 49 }	17·48	8·12	0·71	114	0·34	167	0·09	261
10. 21 7	17·13	7·56	0·64	111	0·28	167	0·01	77
11. 9 28								
11. 21 48	17·22	6·66	0·59	118	0·25	159	0·04	292
12. 10 14								
12. 22 39	16·93	5·40	0·40	122	0·13	166	0·03	77
13. 11 9								
13. 23 39	16·72	4·40	0·33	140	0·05	183	0·00	133
14. 12 23								
15. 1 7	16·72	3·91	0·36	140	0·12	209	0·02	337
15. 13 50								
16. 2 33	16·77	4·30	0·32	145	0·09	204	0·03	356
16. 15 9 } 17. 3 39 }	17·06	4·80	0·33	124	0·08	177	0·01	175
17. 16 5 } 18. 4 30 }	17·11	5·47	0·41	115	0·11	173	0·02	259
18. 16 51 } 19. 5 12 }	17·28	6·15	0·50	113	0·19	163	0·02	109
19. 17 35 } 20. 5 49 }	17·25	6·60	0·47	106	0·22	181	0·04	307
20. 18 3 } 21. 6 19 }	17·04	6·99	0·60	109	0·23	167	0·04	54
21. 18 35 } 22. 6 47 }	16·73	7·10	0·57	111	0·24	175	0·03	350
22. 19 0 } 23. 7 18 }	17·10	7·06	0·59	111	0·30	167	0·02	22

Height of the Water in each individual tide at Limerick, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
June 25. 20 30 / 26. 8 44	18·41	7·82	0·78	90	0·68	153	0·22	199
26. 21 3	17·54	7·93	1·23	86	0·84	119	0·45	155
27. 9 17								
27. 21 30	17·72	7·07	1·02	96	0·64	129	0·24	174
28. 9 46								
28. 22 1	17·50	6·62	1·01	103	0·51	124	0·17	167
29. 10 19								
29. 22 37	17·24	5·92	0·87	97	0·48	129	0·20	167
30. 10 59								
30. 23 20	18·12	5·33	0·71	108	0·34	142	0·10	153
July 1. 11 50								
2. 0 20	17·98	5·03	0·76	118	0·27	151	0·08	225
2. 12 56								
3. 1 31	17·78	5·11	0·92	121	0·43	146	0·25	203
3. 14 16								
4. 3 0	18·45	5·67	0·97	125	0·38	172	0·18	221
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
5. 16 17 / 6. 4 53	17·28	7·18	0·79	106	0·46	155	0·28	191
6. 17 23 / 7. 5 50	18·11	7·97	0·72	97	0·69	143	0·40	204
7. 18 15 / 8. 6 33	18·65	8·32	0·67	95	0·86	148	0·24	221
8. 19 0 / 9. 7 20	18·36	9·21	0·87	90	0·83	136	0·35	173
9. 20 0	18·18	9·78	0·90	66	0·84	123	0·62	138
10. 8 23								
10. 20 45	18·40	9·83	1·02	64	0·84	119	0·63	133
11. 9 6								
11. 21 27	18·00	9·67	1·09	69	0·89	115	0·53	135
12. 9 54								
12. 22 20	18·57	8·69	1·03	80	0·93	130	0·47	160
13. 10 37								
13. 22 54	17·45	8·06	0·89	91	0·82	126	0·54	161
14. 11 19								
14. 23 44	16·91	7·22	0·87	108	0·66	139	0·29	166
15. 12 14								
16. 0 44	17·14	6·27	0·89	124	0·36	172	0·12	244
16. 13 21								
17. 1 58	17·55	5·94	0·73	128	0·30	156	0·11	206
17. 14 35								
18. 3 11	17·38	6·10	0·76	119	0·58	160	0·13	195
18. 15 50 / 19. 4 20	17·38	6·59	0·70	120	0·43	153	0·15	213
19. 16 48 / 20. 5 15	17·45	7·17	0·87	106	0·49	143	0·16	189
20. 17 36 / 21. 5 56	17·46	7·75	0·88	94	0·64	141	0·29	167
21. 18 21 / 22. 6 37	17·50	8·12	1·02	90	0·73	135	0·38	157
22. 18 58 / 23. 7 15	17·58	8·35	1·03	87	0·75	130	0·46	168

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_2)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 19 31 } 24. 7 47 }	17·55	8·40	0·99	85	0·76	138	0·48	163
24. 20 8	17·42	8·50	0·97	76	0·83	111	0·52	138
25. 8 23								
25. 20 37	17·34	8·32	0·94	75	0·70	111	0·46	144
26. 8 51								
26. 21 5	17·10	7·87	0·95	85	0·67	113	0·46	147
27. 9 18								
27. 21 30	17·19	7·31	1·03	88	0·74	122	0·29	160
28. 9 43								
28. 21 55	16·86	6·71	1·11	91	0·59	115	0·40	183
29. 10 14								
29. 22 33	17·21	5·72	0·97	96	0·51	135	0·17	165
30. 10 56								
30. 23 19	16·94	4·95	0·75	110	0·29	142	0·06	214
31. 11 57								
August 1. 0 35	17·11	4·60	0·76	119	0·24	145	0·15	224
1. 13 31								
2. 2 26	17·44	4·31	0·73	113	0·35	165	0·16	264
2. 14 48 } 3. 3 25 }	17·12	5·88	0·75	132	0·39	160	0·08	186
3. 16 3 } 4. 4 38 }	17·65	6·81	0·63	105	0·49	143	0·31	185
4. 17 10 } 5. 5 30 }	18·09	8·10	0·81	86	0·72	148	0·33	171
5. 18 5 } 6. 6 29 }	18·02	9·30	1·09	75	0·81	127	0·51	146
6. 18 50 } 7. 7 10 }	18·41	9·62	1·17	67	0·94	115	0·55	144
7. 19 45	17·99	10·38	0·99	35	0·74	101	0·64	117
8. 8 8								
8. 20 30	18·05	10·36	0·93	40	0·77	100	0·66	118
9. 8 48								
9. 21 6	18·16	9·95	0·96	52	0·91	107	0·69	122
10. 9 27								
10. 21 47	17·73	9·17	0·91	64	0·90	118	0·63	129
11. 10 5								
11. 22 23	17·77	8·24	0·88	85	0·89	122	0·42	152
12. 10 43								
12. 23 3	17·18	6·89	0·87	107	0·62	139	0·22	146
13. 11 31								
13. 23 58	16·87	5·56	0·79	130	0·37	169	0·09	201
14. 12 43								
15. 1 28	16·86	4·95	0·67	134	0·28	160	0·09	210
15. 14 8								
16. 2 47	16·85	5·50	0·75	125	0·39	165	0·13	204
16. 15 29 } 17. 3 56 }	17·20	6·08	0·65	109	0·37	161	0·10	141
17. 16 29 } 18. 4 56 }	17·33	6·87	0·80	106	0·46	143	0·24	195
18. 17 20 } 19. 5 41 }	17·73	7·64	0·94	96	0·62	135	0·30	195
19. 18 7 } 20. 6 21 }	17·84	8·05	1·10	89	0·79	143	0·38	172
20. 18 36 } 21. 6 52 }	17·58	8·49	1·03	80	0·74	133	0·49	150
21. 19 9 } 22. 7 21 }	17·18	8·71	0·98	76	0·74	118	0·51	126
22. 19 35 } 23. 7 53 }	17·63	8·52	1·12	78	0·82	131	0·43	150

Height of the Water in each individual tide at Casleh Bay, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 23. 17 56	18·57	5·70	0·17	211	0·10	233	0·03	354
24. 5 53								
24. 18 31	18·75	5·42	0·12	148	0·17	182	0·04	85
25. 6 41								
25. 19 1	18·14	5·29	0·04	269	0·08	245	0·08	341
26. 7 15								
26. 19 37	17·85	4·88	0·21	180	0·05	118	0·04	87
27. 7 56								
27. 20 15	18·08	4·28	0·15	197	0·09	230	0·07	329
28. 8 35								
28. 20 58	17·80	3·82	0·11	227	0·05	195	0·05	334
29. 9 15								
29. 21 39	17·93	3·33	0·13	192	0·06	240	0·03	16
30. 10 9								
30. 22 32	18·10	3·04	0·04	166	0·07	224	0·04	317
July 1. 11 5								
1. 23 37	17·74	2·77	0·05	254	0·10	188	0·01	213
2. 12 16								
3. 0 55	18·26	2·90	0·03	293	0·06	229	0·03	5
3. 13 26								
4. 1 57	18·83	3·43	0·07	196	0·07	268	0·05	158
4. 14 22								
5. 2 47	17·88	4·06	0·07	208	0·08	252	0·06	341
5. 15 14								
6. 3 40	17·80	4·92	0·06	142	0·13	280	0·03	110
6. 16 3								
7. 4 30	18·35	5·49	0·03	179	0·02	173	0·07	352
7. 16 53								
8. 5 11	18·39	6·18	0·07	288	0·25	239	0·09	347
8. 17 31								
9. 5 51	18·22	6·76	0·08	134	0·15	209	0·06	349
9. 18 16								
10. 6 40	18·21	7·09	0·20	176	0·17	235	0·10	51
10. 18 59								
11. 7 23	18·37	7·06	0·17	165	0·13	246	0·02	267
11. 19 46								
12. 8 9	18·16	6·75	0·20	199	0·08	245	0·07	354
12. 20 40								
13. 9 4	17·94	6·06	0·29	165	0·15	216	0·01	332
13. 21 28								
14. 9 55	17·57	5·12	0·16	195	0·14	180	0·06	345
14. 22 35	17·58	4·49	0·15	235	0·15	240	0·11	58
15. 11 12								
15. 23 48	18·25	3·74	0·14	256	0·07	358	0·04	79
16. 12 24								
17. 1 0	18·27	3·57	0·17	270	0·14	239	0·08	128
17. 13 38								
18. 2 16	18·20	3·80	0·14	247	0·14	234	0·02	93
18. 14 47								
19. 3 18	18·08	4·34	0·07	288	0·04	261	0·01	294
19. 15 42								
20. 4 5	17·98	4·79	0·11	191	0·05	160	0·04	9
20. 16 29								
21. 4 47	17·88	5·12	0·01	275	0·10	247	0·02	258
21. 16 55								
22. 5 11	17·83	5·55	0·07	208	0·06	228	0·08	10
22. 17 29								
23. 5 46	17·87	5·70	0·15	150	0·13	206	0·08	343

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 18 2	17·93	5·70	0·21	154	0·08	230	0·03	289
24. 6 18								
24. 18 32	17·86	5·66	0·19	141	0·05	208	0·07	329
25. 6 52								
25. 19 7	17·71	5·37	0·21	163	0·06	258	0·04	12
26. 7 24								
26. 19 41	17·56	4·97	0·18	176	0·07	196	0·03	329
27. 7 57								
27. 20 13	17·57	4·44	0·16	193	0·08	191	0·06	296
28. 8 28								
28. 20 50	17·44	3·75	0·18	179	0·04	196	0·05	313
29. 9 9								
29. 21 28	17·62	3·35	0·08	157	0·07	233	0·04	318
30. 9 59								
30. 22 29	17·50	2·79	0·05	240	0·04	170	0·05	84
31. 11 8								
31. 23 47	17·71	2·64	0·06	190	0·09	240	0·04	17
August 1. 12 30								
2. 1 12	18·10	2·87	0·09	25	0·12	244	0·03	67
2. 13 49								
3. 2 26	17·73	3·70	0·04	204	0·09	271	0·03	88
3. 14 53								
4. 3 19	17·92	4·80	0·08	206	0·16	258	0·01	333
4. 15 44	18·17	5·52	0·05	259	0·08	246	0·08	34
5. 4 4								
5. 16 30	18·15	6·66	0·18	168	0·23	187	0·05	91
6. 4 54								
6. 17 12	18·29	7·58	0·17	121	0·21	206	0·03	331
7. 5 32								
7. 17 52	18·32	7·86	0·29	137	0·14	208	0·13	343
8. 6 15								
8. 18 39	18·18	7·90	0·33	172	0·14	226	0·09	11
9. 7 0								
9. 19 21	18·08	7·27	0·28	159	0·18	206	0·14	333
10. 7 45								
10. 20 3	17·96	6·30	0·29	169	0·19	198	0·12	337
11. 8 29								
11. 20 56	17·99	5·16	0·19	194	0·14	192	0·05	348
12. 9 19								
12. 21 51	17·83	4·14	0·05	151	0·08	232	0·09	359
13. 10 30								
13. 23 9	17·58	3·26	0·11	275	0·13	290	0·08	90
14. 11 55								
15. 0 40	17·64	2·90	0·06	217	0·05	241	0·05	93
15. 13 21								
16. 2 2	17·73	3·28	0·09	268	0·10	282	0·02	297
16. 14 32								
17. 3 2	17·95	3·81	0·05	254	0·01	268	0·02	350
17. 15 23	17·99	4·29	0·05	117	0·06	219	0·02	238
18. 3 48								
18. 16 3	18·12	4·88	0·08	149	0·13	167	0·04	302
19. 4 24								
19. 16 44	18·12	5·47	0·16	166	0·11	244	0·04	338
20. 4 58								
20. 17 8	17·82	5·87	0·08	150	0·08	236	0·06	8
21. 5 24								
21. 17 40	17·54	6·02	0·11	166	0·07	200	0·08	352
22. 5 52								
22. 18 8	17·91	6·00	0 14	155	0·11	248	0·08	322
23. 6 26								

Height of the Water in each individual tide at Galway, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
June 24. 18 32 / 25. 6 42	18·59	5·40	0·07	106	0·47	177	0·06	149
25. 19 4 / 26. 7 18	17·84	5·46	0·17	173	0·06	231	0·14	352
26. 19 40 / 27. 7 59	17·44	5·06	0·21	183	0·07	175	0·08	354
27. 20 20 / 28. 8 40	17·74	4·40	0·18	200	0·15	199	0 14	24
28. 21 4 / 29. 9 21	17·39	3·94	0·14	214	0·06	169	0·20	331
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
30. 22 36	17·70	3·12	0·09	188	0·08	217	0·13	297
July 1. 11 11 / 1. 23 46	17·58	2·98	0·02	162	0·16	206	0·07	188
2. 12 22 / 3. 0 57	17·94	3·13	0·07	233	0·11	247	0·08	347
3. 13 32 / 4. 2 7	18·51	3·59	0·08	140	0·07	245	0·05	170
4. 14 32 / 5. 2 57	17·56	4·22	0·08	226	0·19	235	0·04	22
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
6. 16 14 / 7. 4 41	17·98	5·58	0·09	96	0·05	237	0·08	316
7. 17 4 / 8. 5 22	18·09	6·34	0·06	156	0·30	208	0·12	336
8. 17 37 / 9. 5 57	17·91	7·03	0·06	90	0·11	203	0·12	2
9. 18 24 / 10. 6 48	17·87	7·32	0·18	158	0·25	223	0·27	28
10. 19 8 / 11. 7 32	17·91	7·38	0·27	178	0·18	249	0·18	358
11. 19 55 / 12. 8 18	17·72	6·99	0·29	177	0·06	221	0·25	359
12. 20 45 / 13. 9 9	17·60	6·18	0·28	186	0·28	193	0·19	347
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
14. 22 40	17·13	4·60	0·16	226	0·25	254	0·13	26
15. 11 15 / 15. 23 50	17·61	3·87	0·22	263	0·16	1	0·06	64
16. 12 24 / 17. 0 58	17·71	3·67	0·27	215	0·21	221	0·07	101
17. 13 37 / 18. 2 15	17·61	3·88	0·06	210	0·19	246	0·08	61
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
19. 15 37 / 20. 4 5	17·52	4·82	0·03	112	0·04	202	0·09	48
20. 16 26 / 21. 4 44	17·31	5·22	0·05	117	0·11	207	0·15	32
21. 17 3 / 22. 5 19	17·36	5·72	0·14	185	0·07	223	0·17	13
22. 17 33 / 23. 5 50	17·43	5·86	0·17	143	0·20	194	0·23	317

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 18 5 / 24. 6 21	17·33	6·01	0·20	162	0·22	208	0·11	331
24. 18 34 / 25. 6 54	17·32	5·88	0·18	131	0·07	166	0·21	308
25. 19 7 / 26. 7 25	17·22	5·52	0·27	149	0 08	226	0·15	339
26. 19 33 / 27. 7 49	16·96	5·20	0·12	201	0·21	203	0·08	319
27. 20 12 / 28. 8 27	16·98	4·65	0·18	193	0·18	182	0·05	340
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
29. 21 33	17·09	3·50	0·21	162	0·18	239	0·14	312
30. 10 6								
30. 22 39	17·04	2·87	0·06	241	0·09	239	0·14	56
31. 11 20								
August 1. 0 0	17·25	2·72	0·03	284	0·08	213	0·02	15
1. 12 39								
2. 1 18	17·66	3·12	0·05	128	0·13	243	0·02	321
2. 13 50								
3. 2 22	17·37	3·93	0·07	174	0·11	245	0·02	99
3. 14 48								
4. 3 14	17·62	4·95	0·08	139	0·20	247	0·07	302
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
5. 16 26 / 6. 4 50	18·05	6·84	0·16	150	0·23	187	0·11	1
6. 17 18 / 7. 5 38	18·02	7·93	0·30	118	0·32	200	0·16	4
7. 17 59 / 8. 6 22	17·93	8·13	0·25	145	0·25	195	0·20	9
8. 18 51 / 9. 7 12	17·77	8·24	0·43	183	0·25	202	0·25	344
9. 19 34 / 10. 7 58	17·62	7·65	0·29	160	0·30	216	0·26	7
10. 20 13 / 11. 8 39	17·56	6·50	0·40	168	0·29	199	0·21	342
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
12. 22 3	17·21	4·52	0·09	166	0·22	200	0·04	83
13. 10 43								
13. 23 22	17·20	3·43	0·13	259	0·11	276	0·09	124
14. 12 7								
15. 0 52	17·12	3·05	0·02	27	0·05	198	0·10	116
15. 13 33								
16. 2 14	17·23	3·38	0·03	234	0·16	290	0·05	314
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
17. 15 35 / 18. 3 59	17·43	4·46	0·02	154	0·11	188	0·09	291
18. 16 15 / 19. 4 36	17·79	5·18	0·16	127	0·14	175	0·18	277
19. 16 51 / 20. 5 5	17·66	5·67	0·18	160	0·17	209	0·09	53
20. 17 15 / 21. 5 31	17·37	6·18	0·13	154	0·07	213	0·10	330
21. 17 43 / 22. 5 55	17·06	6·30	0·19	163	0·11	159	0·25	348
22. 18 6 / 23. 6 24	17·49	6·22	0·26	148	0·11	204	0·27	313

Height of the Water in each individual tide at Old Head, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 22. 17 15	18·51	5·11	0·12	133	0·08	331	0·08	298
23. 5 39								
23. 18 3	18·55	5·04	0·16	146	0·09	23	0·05	315
24. 6 23								
24. 18 40	18·49	4·73	0·12	207	0·17	222	0·09	44
25. 6 50								
25. 19 13	17·88	4·69	0·08	186	0·07	197	0·07	185
26. 7 27								
26. 20 3	17·53	4·40	0·12	192	0·03	28	0·08	342
27. 8 22								
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
28. 21 27	17·73	3·58	0·09	356	0·06	174	0·02	116
29. 9 50								
29. 22 12	17·65	3·07	0·06	348	0·09	160	0·03	60
30. 10 32								
30. 22 52	17·91	2·68	0·04	276	0·12	173	0·02	328
July 1. 11 25								
1. 23 58	17·69	2·52	0·08	207	0·09	195	0·05	210
2. 12 35								
3. 1 12	18·02	2·65	0·05	98	0·06	230	0·04	244
3. 13 43								
4. 2 14	18·68	3·08	0·05	218	0·10	231	0·02	39
4. 14 42								
5. 3 9	17·86	3·71	0·12	161	0·15	280	0·04	57
5. 15 21	17·43	4·12	0·19	327	0·16	293	0·15	220
6. 3 55								
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
7. 17 12	18·05	5·43	0·09	332	0·21	213	0·07	339
8. 5 30								
8. 17 52	18·23	6·12	0·13	93	0·14	222	0·09	316
9. 6 12								
9. 18 36	18·22	6·52	0·21	112	0·15	159	0·11	276
10. 7 0								
10. 19 18	18·15	6·29	0·12	194	0·17	186	0·05	303
11. 7 42								
11. 20 7	17·93	6·13	0·15	233	0·08	358	0·02	335
12. 8 30								
12. 20 56	17·83	5·41	0·16	167	0·01	128	0·03	245
13. 9 20								
13. 21 51	17·31	4·93	0·08	264	0·02	169	0·08	278
14. 10 19								
14. 22 47	17·52	4·07	0·10	206	0·08	270	0·06	267
15. 11 22								
15. 23 56	18·10	3·47	0·10	273	0·15	314	0·03	175
16. 12 32								
17. 1 7	18·08	3·21	0·16	249	0·25	275	0·11	215
17. 13 44								
18. 2 21	17·95	3·37	0·11	178	0·07	251	0·04	356
18. 14 52								
19. 3 23	17·88	3·91	0·02	326	0·16	289	0·07	21
19. 15 44	17·83	4·18	0·08	10	0·17	237	0·05	167
20. 4 11								
20. 16 35	17·61	4·59	0·04	120	0·07	279	0·04	113
21. 4 53								
21. 17 14	17·56	4·93	0·07	201	0·11	292	0·07	227
22. 5 30								
22. 17 44	17·61	5·10	0·05	112	0·07	272	0·06	311
23. 6 1								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 18 15	17·57	5·09	0·06	147	0·13	195	0·08	259
24. 6 31								
24. 18 44	17·72	5·10	0·18	118	0·05	127	0·02	273
25. 7 4								
25. 19 20	17·51	4·81	0·15	151	0·09	72	0·03	240
26. 7 37								
26. 19 51	17·42	4·52	0·12	189	0·05	110	0·08	254
27. 8 7								
27. 20 30	17·42	3·83	0·18	184	0·18	118	0·06	284
28. 8 45								
28. 21 15	17·26	3·63	0·07	139	0·11	124	0·09	138
29. 9 39								
29. 22 3	17·35	2·95	0·06	156	0·01	267	0·05	158
30. 10 30								
30. 22 57	17·29	2·47	0·02	208	0·09	267	0·05	12
31. 11 26								
August 1. 0 15	17·56	2·30	0·04	204	0·19	267	0·01	6
1. 12 57								
2. 1 38	17·85	2·57	0·15	201	0·29	275	0·12	91
2. 14 12								
3. 2 46	17·55	3·32	0·10	257	0·27	282	0·08	281
3. 15 2								
4. 3 18	17·64	4·12	0·10	206	0·11	248	0·09	262
4. 15 49	18·32	4·97	0·15	33	0·10	242	0·12	220
5. 4 9								
5. 16 32	18·20	5·96	0·08	51	0·13	260	0·13	337
6. 4 56								
....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
8. 19 5	17·98	7·13	0·17	167	0·07	120	0·10	180
9. 7 26								
9. 19 45	17·92	6·55	0·15	171	0·07	324	0·07	268
10. 8 9								
10. 20 28	17·66	5·75	0·12	148	0·02	33	0·01	183
11. 8 54								
11. 21 19	17·70	4·86	0·07	198	0·03	157	0·06	103
12. 9 42								
12. 22 10	17·57	3·71	0·21	199	0·26	229	0·05	253
13. 10 47								
13. 23 23	17·44	3·00	0·06	225	0·13	238	0·03	83
14. 12 11								
15. 0 58	17·33	2·48	0·08	239	0·18	276	0·03	157
15. 13 37								
16. 2 16	17·52	2·67	0·13	172	0·19	270	0·02	13
16. 14 50								
17. 3 23	17·79	3·36	0·04	359	0·16	277	0·06	27
17. 15 36	17·69	3·98	0·08	308	0·06	216	0·01	93
18. 4 5								
18. 16 26	17·90	4·52	0·09	107	0·02	75	0·09	169
19. 4 47								
19. 17 3	18·07	5·03	0·17	79	0·07	278	0·03	134
20. 5 17								
20. 17 51	17·18	5·27	0·31	266	0·03	159	0·10	194
21. 6 7								
21. 18 12	17·67	5·52	0·27	70	0·13	91	0·12	257
22. 6 24								
22. 18 38	17·69	5·28	0·03	155	0·09	73	0·04	310
23. 6 56								

Height of the Water in each individual tide at Mullaghmore, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 22. 17 45	18·44	4·98	0·17	246	0·04	72	0·08	189
23. 5 45								
23. 18 17	18·85	4·97	0·18	187	0·14	65	0·13	237
24. 6 39								
24. 18 46	19·12	4·77	0·17	27	0·25	274	0·33	108
25. 6 56								
25. 19 34	18·29	4·51	0·19	265	0·18	16	0·07	328
26. 7 48								
26. 20 10	18·01	4·10	0·13	157	0·15	30	0·10	261
27. 8 29								
27. 20 50	18·20	3·77	0·04	228	0·11	60	0·02	175
28. 9 7								
28. 21 24	18·25	3·32	0·16	115	0·12	327	0·07	282
29. 9 49								
29. 22 14	18·08	2·90	0·11	126	0·10	283	0·06	131
30. 10 40								
30. 23 6	18·33	2·56	0·09	224	0·02	244	0·12	204
July 1. 11 51								
2. 0 36	18·46	2·51	0·09	95	0·12	234	0·05	50
2. 13 3								
3. 1 29	18·35	2·46	0·04	178	0·08	160	0·05	315
3. 14 8								
4. 2 46	18·97	2·87	0·22	358	0·10	151	0·05	18
4. 15 3	18·26	3·35	0·11	321	0·13	250	0·07	22
5. 3 21								
5. 15 35	17·96	3·88	0·04	149	0·04	126	0·05	265
6. 4 19								
6. 16 37	18·87	4·27	0·26	129	0·19	355	0·04	264
7. 5 4								
7. 17 18	18·42	5·11	0·33	291	0·19	275	0·11	171
8. 5 36								
8. 17 59	18·73	5·77	0·10	172	0·09	28	0·12	173
9. 6 19								
9. 18 53	18·65	6·02	0·22	166	0·19	16	0·08	260
10. 7 17								
10. 19 30	18·57	5·89	0·13	148	0·05	64	0·10	238
11. 7 54								
11. 20 18	18·01	5·68	0·15	204	0·17	56	0·06	188
12. 8 41								
12. 21 10	18·04	5·41	0·05	333	0·24	33	0·06	44
13. 9 38								
13. 22 6	17·61	4·59	0·17	241	0·13	45	0·08	204
14. 10 32								
14. 22 57	17·73	3·92	0·11	156	0·16	121	0·15	212
15. 11 32								
16. 0 6	18·15	3·16	0·25	276	0·12	152	0·09	246
16. 12 44								
17. 1 22	18·27	3·12	0·21	158	0·10	81	0·04	151
17. 14 5								
18. 2 47	18·22	3·15	0·09	176	0·04	68	0·02	112
18. 15 17								
19. 3 46	18·05	3·40	0·10	274	0·14	58	0·12	56
19. 16 12	18·09	3·73	0·14	254	0·12	227	0·05	199
20. 4 39								
20. 16 57	18·03	4·21	0·01	128	0·04	75	0·02	51
21. 5 15								
21. 17 42	17·68	4·50	0·30	223	0·17	217	0·17	176
22. 5 58								
22. 18 3	17·87	4·85	0·07	51	0·13	46	0·07	192
23. 6 20								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 18 39	17·91	5·01	0·05	78	0·16	41	0·05	142
24. 6 55								
24. 19 11	17·99	4·81	0·19	117	0·10	298	0·04	120
25. 7 31								
25. 19 40	17·62	4·65	0·14	240	0·16	59	0·12	207
26. 7 57								
26. 20 14	17·63	4·18	0·13	114	0·11	30	0·05	159
27. 8 30								
27. 20 51	17·77	3·80	0·07	180	0·08	352	0·08	179
28. 9 4								
28. 21 17	17·57	3·56	0·09	101	0·07	73	0·15	68
29. 9 45								
29. 22 12	17·64	2·82	0·04	190	0·09	136	0·05	324
30. 10 38								
30. 23 4	17·50	2·19	0 02	150	0·01	130	0·08	212
31. 11 49								
August 1. 0 33	17·71	2·07	0·04	157	0·05	139	0·03	336
1. 13 20								
2. 2 6	18·24	2·45	0·05	232	0·06	113	0·04	30
2. 14 36								
3. 3 5	17·93	3·15	0·11	168	0·17	253	0·05	285
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
4. 16 20	18·40	4·69	0 05	337	0·06	14	0·05	152
5. 4 40								
5. 17 12	18·64	5·53	0·26	112	0·09	298	0·04	62
6. 5 36								
6. 17 56	18·73	6·18	0·12	84	0·14	301	0·08	90
7. 6 16								
7. 18 17	18·13	6·54	0·41	340	0·18	227	0·25	127
8. 6 40								
8. 19 15	18·30	6·68	0·27	174	0·13	51	0·15	214
9. 7 36								
9. 19 59	18·54	6·29	0·33	138	0·17	26	0·10	194
10. 8 23								
10. 20 49	17·97	5·57	0·16	233	0·19	38	0·09	241
11. 9 13								
11. 21 36	18·15	4·71	0·07	202	0·16	49	0·10	220
12. 10 2								
12. 22 27	18·02	3·62	0·10	263	0·10	75	0·11	227
13. 11 7								
13. 23 47	17·79	2·88	0·05	220	0·18	164	0·09	245
14. 12 31								
15. 1 14	17·64	2·31	0·10	181	0·09	175	0·12	37
15. 13 58								
16. 2 41	17·70	2·72	0·08	225	0·07	208	0·13	3
16. 15 10								
17. 3 38	17·99	3·27	0·08	212	0·06	97	0·05	96
17. 15 56	17·99	3·73	0·07	257	0·05	211	0·11	190
18. 4 12								
18. 16 42	18·26	4·18	0·02	188	0·13	9	0·10	293
19. 5 3								
19. 17 15	18·52	4·53	0·03	320	0·16	352	0·03	5
20. 5 29								
20. 17 26	17·61	4·79	0·29	216	0·15	170	0·12	184
21. 5 42								
21. 18 29	17·89	5 39	0·29	121	0·26	1	0·07	52
22. 6 41								
22. 19 45	17·86	5·32	0·26	217	0·06	101	0·15	134
23. 8 3								

Height of the Water in each individual tide at Buncrana, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 22. 18 2	17·23	5·28	0·18	184	0·12	263	0·04	113
23. 6 18								
23. 18 38	18·38	5·10	0·16	145	0·19	234	0·06	158
24. 6 51								
24. 19 23	18·22	5·10	0·12	215	0·16	266	0·05	259
25. 7 38								
25. 20 3	17·93	4·69	0·11	123	0·12	263	0·03	158
26. 8 20								
26. 20 35	17·44	4·37	0·20	161	0·09	281	0·05	282
27. 9 7								
27. 21 15	17·66	3·69	0·13	203	0·15	248	0·07	230
28. 9 20								
28. 22 0	17·47	3·22	0·09	164	0·11	256	0·00	74
29. 10 25								
29. 22 48	17·40	2·97	0·15	54	0·03	203	0·04	143
30. 11 17								
30. 23 46	17·71	2·64	0·15	79	0·04	244	0·03	203
July 1. 12 20								
2. 0 54	17·52	2·42	0·12	100	0·07	243	0·02	260
2. 13 32								
3. 2 10	17·70	2·58	0·12	128	0·01	264	0·01	322
3. 14 44								
4. 3 18	18·18	3·04	0·17	186	0·11	322	0·02	97
4. 15 48	17·79	3·39	0·22	261	0·05	199	0·06	159
5. 4 3								
5. 16 26	17·06	4·18	0·08	280	0·05	300	0·03	279
6. 5 4								
6. 17 24	17·55	4·94	0·08	289	0·09	252	0·04	167
7. 5 37								
7. 18 4	17·70	5·75	0·10	193	0·05	283	0·03	221
8. 6 29								
8. 18 52	17·78	6·25	0·23	153	0·17	261	0·04	340
9. 7 16								
9. 19 33	17·85	6·57	0·28	163	0·11	322	0·09	296
10. 8 0								
10. 20 21	17·98	6·56	0·29	152	0·08	237	0·11	253
11. 8 45								
11. 21 11	17·32	6·10	0·24	177	0·04	253	0·11	328
12. 9 29								
12. 22 5	17·31	5·79	0·26	215	0·15	265	0·07	241
13. 10 33								
13. 23 0	16·99	4·91	0·15	253	0·13	296	0·07	290
14. 11 31								
15. 0 2	16·81	4·06	0·13	227	0·02	359	0·02	208
15. 12 38								
16. 1 13	17·31	3·31	0·09	204	0·07	320	0·02	132
16. 13 55								
17. 2 36	17·53	3·15	0·07	177	0·03	263	0·05	143
17. 15 15								
18. 3 54	17·28	3·32	0·21	211	0·03	162	0·00	29
18. 16 22	17·29	3·70	0·07	269	0·02	26	0·04	287
19. 5 2								
19. 17 19	17·28	4·12	0·01	78	0·06	241	0·03	325
20. 5 43								
20. 18 5	17·16	4·61	0·10	137	0·03	311	0·06	207
21. 6 22								
21. 18 39	17·04	5·02	0·09	159	0·08	246	0·04	238
22. 6 56								
22. 19 10	17·16	5·21	0·12	106	0 08	256	0·05	345
23. 7 24								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 19 41	17·21	5·23	0·12	149	0·07	216	0·08	264
24. 7 53								
24. 20 3	17·26	5·11	0·14	118	0·09	224	0·03	259
25. 8 20								
25. 20 37	16·96	4·87	0·05	108	0·07	194	0·01	325
26. 8 44								
26. 21 3	16·87	4·41	0·03	135	0·06	213	0·02	306
27. 9 23								
27. 21 38	17·04	3·87	0·05	159	0·07	217	0·02	266
28. 9 51								
28. 22 9	16·82	3·53	0·04	311	0·13	247	0·04	235
29. 10 29								
29. 22 49	17·02	2·86	0·11	58	0·08	234	0·02	179
30. 11 18								
30. 23 46	17·06	2·41	0·12	27	0·06	190	0·02	15
31. 12 31								
August 1. 1 16	17·24	2·25	0·14	126	0·01	284	0·01	95
1. 14 1								
2. 2 45	17·68	2·65	0·18	189	0·10	225	0·05	234
2. 15 9	17·24	3·10	0·14	227	0·04	216	0·03	53
3. 3 45								
3. 16 13	17·60	3·87	0·05	292	0·09	195	0·05	199
4. 4 36								
4. 17 3	17·79	5·20	0·14	249	0·08	233	0·08	272
5. 5 23								
5. 17 43	17·77	6·20	0·12	210	0·06	102	0·14	294
6. 6 4								
6. 18 27	17·82	6·90	0·10	189	0·06	258	0·02	237
7. 6 42								
7. 19 6	17·67	7·42	0·25	151	0·34	280	0·08	56
8. 7 31								
8. 19 49	17·71	7·40	0·25	178	0·11	285	0·12	309
9. 8 11								
9. 20 32	17·71	6·68	0·26	168	0·12	300	0·08	313
10. 8 53								
10. 21 16	17·37	5·76	0·29	158	0·09	293	0·10	294
11. 9 37								
11. 22 16	17·22	4·92	0·03	254	0·06	328	0·03	310
12. 10 43								
12. 23 10	17·32	3·62	0·10	244	0·09	266	0·04	313
13. 11 50								
14. 0 30	17·09	2·76	0·08	146	0·06	325	0·03	154
14. 13 17								
15. 2 3	16·96	2·50	0·13	208	0·04	69	0·01	315
15. 14 43								
16. 3 22	16·99	2·81	0·11	217	0·03	90	0·02	96
16. 15 53	17·46	3·32	0·01	172	0·06	244	0·04	165
17. 4 18								
17. 16 48	17·37	3·88	0·01	169	0·03	196	0·03	209
18. 5 4								
18. 17 22	17·54	4·52	0·09	161	0·07	273	0·02	9
19. 5 39								
19. 17 52	17·70	5·04	0·01	202	0·06	185	0·05	235
20. 6 4								
20. 18 22	17·33	5·38	0·17	156	0·08	213	0·06	237
21. 6 37								
21. 18 49	17·11	5·50	0·17	129	0·08	266	0·09	330
22. 7 4								
22. 19 15	17·67	5·46	0·06	148	0·05	292	0·05	302
23. 7 36								

Height of the Water in each individual tide at Port Rush, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 22. 18 35	17·91	2·38	0·05	195	0·10	266	0·04	163
23. 6 51								
23. 19 3	18·62	2·22	0·03	229	0·14	251	0·05	132
24. 7 16								
24. 19 37	18·64	2·21	0·06	185	0·14	250	0·03	342
25. 7 52								
25. 20 16	18·26	1·89	0·05	141	0·12	267	0·03	323
26. 8 33								
26. 20 21	17·66	1·62	0·08	154	0·08	248	0·02	313
27. 8 56								
27. 21 43	17·92	1 39	0·11	221	0·12	239	0·01	171
28. 9 48								
28. 22 30	17·52	1·20	0·09	255	0·10	247	0·02	117
29. 10 55								
29. 23 30	17·41	1·03	0·08	324	0·12	290	0·04	226
30. 12 30								
July 1. 0 39	17·95	0·87	0·07	84	0·05	310	0·00	156
1. 13 23								
2. 2 7	17·86	0·94	0·14	161	0·06	310	0·02	106
2. 14 58								
3. 3 48	17·93	1·13	0·11	172	0·04	338	0·01	66
3. 16 12								
4. 4 36	18·46	1·38	0·15	215	0·07	315	0·02	117
4. 16 57								
5. 5 18	18·10	1·57	0·12	239	0·11	274	0 05	155
5. 17 8	17·35	1·76	0·06	223	0·12	252	0·04	104
6. 5 46								
6. 17 49	17·88	2·26	0·12	283	0·10	243	0·03	176
7. 6 2								
7. 18 27	18·00	2·60	0·06	193	0·15	274	0·02	150
8. 6 52								
8. 19 0	18·15	2·80	0·15	164	0 07	270	0·01	314
9. 7 24								
9. 19 38	18·28	2·90	0·14	158	0·14	265	0·02	334
10. 8 5								
10. 20 28	18·29	2·91	0·15	155	0·16	258	0·02	162
11. 8 51								
11. 21 12	17·83	2·58	0·18	143	0·11	260	0·01	355
12. 9 30								
12. 21 52	18·15	2·17	0·22	153	0·11	227	0·01	56
13. 10 18								
13. 22 47	17·37	1·72	0·21	150	0·09	254	0·02	105
14. 11 25								
15. 0 29	17·04	1·52	0·14	282	0·09	314	0·04	158
15. 13 12								
16. 1 55	17·52	1·21	0·07	276	0·05	335	0·04	169
16. 14 39								
17. 3 23	17·73	1·31	0·06	237	0 03	322	0·06	206
17. 15 58								
18. 4 32	17·51	1·41	0·14	227	0·08	285	0·01	156
18. 16 57								
19. 5 22	17·41	1·68	0·13	277	0·06	312	0·02	189
19. 17 40	17·56	1·82	0·01	29	0·06	274	0·02	176
20. 6 4								
20. 18 15	17·57	2·00	0·05	69	0·13	287	0·00	180
21. 6 32								
21. 18 46	17·47	2·14	0·02	56	0·12	275	0·01	253
22. 7 23								
22. 19 19	17·57	2·22	0·11	116	0·11	274	0·03	285
23. 7 33								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 19 31	17·57	2·33	0·12	346	0·20	240	0·08	141
24. 7 43								
24. 20 10	17·52	2·15	0·06	85	0·14	261	0·03	157
25. 8 27								
25. 20 49	17·37	1·98	0·04	201	0·13	256	0·02	144
26. 8 56								
26. 21 15	17·22	1·70	0·01	61	0·10	252	0·02	135
27. 9 35								
27. 21 57	17·38	1·39	0·02	70	0·10	246	0·04	239
28. 10 10								
28. 22 49	17·06	1·17	0·07	31	0·10	256	0 03	126
29. 11 16								
29. 23 43	17·18	0·93	0·10	64	0·05	310	0·01	183
30. 12 29								
31. 1 15	17·40	0·80	0·15	78	0·03	323	0·02	330
31. 14 11								
August 1. 3 6	17·43	0·94	0·17	173	0·06	329	0·00	17
1. 15 41								
2. 4 16	17·84	1·30	0·10	201	0·09	305	0·03	241
2. 16 20	17·34	1·39	0·20	260	0·08	222	0·04	173
3. 4 56								
3. 17 8	17·77	1·79	0·16	273	0·11	254	0·04	184
4. 5 31								
4. 17 42	18·16	2·28	0·15	267	0·17	242	0·05	242
5. 6 2								
5. 18 11	18·00	2·72	0·08	201	0·16	239	0·03	129
6. 6 32								
6. 18 47	18·19	3·17	0·07	179	0·20	245	0·03	322
7. 7 2								
7. 19 19	18·06	3·48	0·23	144	0·13	248	0·03	307
8. 7 44								
8. 19 59	18·03	3·32	0·20	161	0·10	250	0·02	321
9. 8 21								
9. 20 45	17·91	2·91	0·20	155	0·14	247	0·03	226
10. 9 6								
10. 21 27	17·57	2·37	0·21	144	0·11	266	0·02	310
11. 9 48								
11. 22 19	17·83	1·62	0·19	154	0·12	257	0·02	39
12. 10 54								
13. 0 1	17·58	1·27	0·11	319	0·06	302	0·04	163
13. 12 55								
14. 1 49	17·39	1·00	0·07	239	0·04	7	0·03	208
14. 14 35								
15. 3 21	17·23	1·06	0·10	244	0·01	82	0·04	181
15. 15 55								
16. 4 28	17·21	1·30	0·06	261	0·06	330	0·03	215
16. 16 50	17·68	1·46	0·06	77	0·09	300	0·03	239
17. 5 15								
17. 17 33	17·62	1·67	0·02	67	0·09	293	0·04	56
18. 5 49								
18. 18 2	17·88	2·03	0·04	173	0·11	277	0·03	187
19. 6 19								
19. 18 13	18·16	2·16	0·09	125	0·18	226	0·06	305
20. 6 25								
20. 18 45	17·60	2·28	0·04	140	0·12	251	0·04	199
21. 7 0								
21. 19 17	17·36	2·42	0·06	129	0·11	286	0·02	215
22. 7 32								
22. 19 43	17·89	2·29	0·04	288	0·11	262	0·03	225
23. 7 57								

Height of the Water in each individual tide at Carrowkeel, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 22. 19 42 } 23. 7 58	18·10	3·11	0·13	94	0·05	146	0·06	349
23. 20 29 } 24. 8 42	18·69	2·90	0·03	23	0·06	210	0·05	206
24. 21 7 } 25. 9 22	18·58	2·95	0·03	276	0·06	223	0·01	314
25. 21 29 } 26. 9 46	18·26	2·70	0·08	97	0·05	182	0·02	37
26. 21 50 } 27. 10 22	17·82	2·42	0·05	152	0·07	169	0·02	247
27. 22 42 } 28. 10 47	17·90	2·15	0·18	208	0·07	198	0·03	305
28. 23 3	17·92	2·02	0·10	28	0·09	157	0·05	192
29. 11 34								
30. 0 4	17·98	1·60	0·12	5	0·06	161	0·01	57
30. 12 36								
July 1. 1 8	18·34	1·39	0·21	58	0·05	188	0·03	269
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
3. 4 4	18·35	1·58	0·21	125	0·12	221	0·04	33
3. 16 13 } 4. 4 49	18·58	1·61	0·07	211	0·15	220	0·08	45
4. 17 14 } 5. 5 29	18·54	2·15	0·09	207	0·12	194	0·03	157
5. 17 45 } 6. 6 22	17·75	2·54	0·07	225	0·11	169	0·03	346
6. 18 45 } 7. 6 58	18·41	2·99	0·10	269	0·12	222	0·04	49
7. 19 28 } 8. 7 53	18·61	3·50	0·10	13	0·14	235	0·06	346
8. 20 5 } 9. 8 29	18·52	3·60	0·09	179	0·05	196	0·02	341
9. 20 54	18·36	3·80	0·12	252	0·09	198	0·06	97
10. 9 23								
10. 21 52	18·48	3·80	0·25	248	0·11	209	0·06	218
11. 10 13								
11. 22 34	17·91	3·52	0·16	234	0·05	171	0·09	60
12. 10 57								
12. 23 20	18·02	3·25	0·19	270	0·07	279	0·03	172
13. 11 47								
14. 0 14	17·46	2·69	0·23	290	0·04	290	0·09	123
14. 12 45								
15. 1 15	17·41	2·21	0·17	283	0·03	116	0·06	86
15. 13 50								
16. 2 25	17·99	1·88	0·09	291	0·03	343	0·06	145
16. 15 5								
17. 3 45	18·46	1·92	0·08	112	0·05	323	0·03	281
17. 16 22 } 18. 5 5	18·40	2·06	0·13	139	0·06	193	0·05	178
18. 17 32 } 19. 6 12	18·24	2·33	0·12	278	0·05	239	0·05	312
19. 18 25 } 20. 6 49	18·14	2·58	0·08	101	0·10	228	0·10	9
20. 19 7 } 21. 7 24	18·01	2·80	0·08	169	0·11	183	0·06	74
21. 19 35 } 22. 7 52	17·92	2·97	0·07	349	0·17	200	0·00	115
22. 20 14 } 23. 8 28	17·98	3·09	0·03	217	0·12	203	0·05	288

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 20 46	17·92	2·95	0·04	231	0·17	197	0·03	230
24. 8 58								
24. 21 8	17·96	2·88	0·07	101	0·13	197	0·01	5
25. 9 25								
25. 21 43	17·79	2·82	0·02	342	0·10	166	0·03	75
26. 9 56								
26. 22 9	17·55	2·62	0·15	316	0·08	181	0·00	218
27. 10 28								
27. 22 47	17·60	2·24	0·07	266	0·09	201	0·02	255
28. 11 4								
28. 23 21	17·56	1·92	0·08	307	0·08	201	0·01	65
29. 11 44								
30. 0 6	17·50	1·62	0·15	9	0·14	169	0·04	179
30. 12 42								
31. 1 17	17·69	1·30	0·20	20	0·08	187	0·05	197
31. 14 15								
August 1. 3 13	17·97	1·42	0·33	119	0·03	222	0·03	293
1. 15 30	17·98	1·67	0·25	207	0·13	156	0·05	90
2. 3 56								
2. 16 30	17·79	1·92	0·19	218	0·06	163	0·03	289
3. 5 6								
3. 17 39	18·22	2·52	0·14	255	0·13	188	0·04	9
4. 6 2								
4. 18 43	18·39	3·07	0·14	242	0·18	202	0·03	26
5. 7 3								
5. 19 15	18·43	3·67	0·12	187	0·11	208	0·02	104
6. 7 36								
6. 19 53	18·59	4·04	0·10	323	0·12	205	0·04	121
7. 8 8								
7. 20 39	18·60	4·21	0·12	144	0·17	201	0·11	337
8. 9 4								
8. 21 33	18·42	4·16	0·22	224	0·22	202	0·04	342
9. 9 55								
9. 22 17	18·23	3·80	0·16	248	0·19	227	0·13	211
10. 10 35								
10. 22 52	17·79	3·32	0·10	257	0·17	215	0·02	356
11. 11 18								
11. 23 44	17·76	2·65	0·15	271	0·07	193	0·07	155
12. 12 7								
13. 0 29	17·62	1·95	0·12	305	0·04	201	0·03	131
13. 13 15								
14. 2 1	17·52	1·54	0·05	205	0·03	285	0·02	142
14. 14 50								
15. 3 38	17·60	1·63	0·10	146	0·03	131	0·03	155
15. 16 26	17·77	1·78	0·12	106	0·11	226	0·03	212
16. 5 2								
16. 17 28	18·09	2·08	0·05	149	0·05	235	0·01	341
17. 5 53								
17. 18 13	18·07	2·40	0·08	188	0·09	183	0·05	216
18. 6 29								
18. 18 52	18·24	2·72	0·02	224	0·15	228	0·04	189
19. 7 9								
19. 19 14	18·33	3·09	0·09	261	0·14	203	0·07	70
20. 7 26								
20. 19 50	17·93	3·08	0·02	281	0·11	188	0·05	345
21. 8 5								
21. 20 19	17·66	3·18	0·05	47	0·16	176	0·05	99
22. 8 34								
22. 20 40	18·10	2·10	0·09	149	0·24	178	0·04	110
.....	....	....	....	....	....	....	....	....

Height of the Water in each individual tide at Ballycastle, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1852. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
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.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
June 27. 23 3	17·45	0·95	0·08	2	0·08	260	0·04	156
28. 11 48								
29. 0 34	17·35	0·62	0·18	73	0·09	315	0·04	268
29. 13 20								
30. 2 6	17·24	0·64	0·15	139	0·04	70	0·02	349
30. 15 1								
July 1. 3 56	17·57	0·69	0·09	161	0·00	284	0·05	71
1. 16 3	17·29	0·78	0·17	270	0·05	157	0·04	10
2. 4 21								
2. 16 55	17·30	0·94	0·12	308	0·06	251	0·00	55
3. 5 25								
3. 17 56	17·94	0·94	0·13	341	0·02	3	0·02	46
4. 6 17								
4. 18 15	18·10	0·98	0·14	307	0·06	183	0·05	198
5. 6 24								
5. 18 39	17·16	1·08	0·13	349	0·06	302	0·01	269
6. 6 59								
6. 19 4	17·77	1·33	0·25	318	0·08	281	0·06	276
7. 6 49								
7. 19 13	17·82	1·52	0·06	321	0·15	271	0·06	250
8. 7 22								
8. 19 33	17·88	1·57	0·06	267	0·11	235	0·03	204
9. 7 47								
9. 20 16	18·00	1·62	0·09	232	0·14	252	0·04	133
10. 8 34								
10. 20 45	18·12	1·62	0·05	82	0·14	272	0·06	221
11. 9 10								
11. 21 28	17·59	1·45	0·12	216	0·11	240	0·02	11
12. 9 52								
12. 23 14	17·66	1·32	0·29	314	0·09	359	0·11	205
13. 11 42								
14. 0 9	17·14	0·86	0·27	293	0·19	321	0·10	143
14. 12 41								
15. 1 13	16·87	0·71	0·23	284	0·11	338	0·08	76
15. 14 14								
16. 3 15	17·45	0·70	0·03	37	0·03	23	0·05	261
16. 16 10	17·81	1·02	0·06	115	0·04	276	0·07	209
17. 4 58								
17. 17 23	17·56	1·08	0·10	290	0·03	47	0·03	284
18. 5 48								
18. 18 35	17·48	1·16	0·12	33	0·06	297	0·05	321
19. 6 42								
19. 18 55	17·50	1·13	0·12	40	0·08	323	0·11	299
20. 6 56								
20. 19 6	17·39	1·18	0·10	25	0·10	309	0·06	272
21. 7 13								
21. 19 36	17·30	1·27	0·07	11	0·06	317	0·01	325
22. 7 50								
22. 20 6	17·34	1·29	0·10	28	0·12	293	0·02	291
23. 8 16								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 20 28	17·29	1·24	0·06	2	0·12	300	0·04	234
24. 8 38								
24. 20 49	17·25	1·20	0·13	339	0·14	293	0·04	213
25. 9 9								
25. 21 46	17·11	0·97	0·13	329	0·09	314	0·03	182
26. 10 3								
26. 22 19	16·98	0·83	0·13	342	0·12	319	0·00	250
27. 10 50								
27. 23 21	17·23	0·70	0·08	10	0·10	341	0·02	240
28. 11 51								
29. 0 21	17·06	0·56	0·10	303	0·04	350	0·01	242
29. 13 10								
30. 2 0	17·17	0·63	0·12	113	0·04	171	0·03	343
30. 14 37	17·11	0·78	0·13	192	0·07	153	0·01	109
31. 3 11								
31. 15 24	17·07	0·92	0·19	263	0·05	214	0·03	63
August 1. 4 12								
1. 17 1	17·49	0·91	0·22	317	0·06	281	0·00	273
2. 5 5								
2. 17 57	17·31	0·97	0·09	17	0·04	2	0·02	91
3. 6 19								
3. 18 22	17·78	1·06	0·11	339	0·07	293	0·07	270
4. 6 29								
4. 18 18	18·06	1·36	0·15	315	0·08	199	0·08	227
5. 6 31								
5. 18 49	17·87	1·65	0·09	280	0·13	288	0·05	242
6. 7 6								
6. 19 16	18·11	1·87	0·08	274	0·17	233	0·08	189
7. 7 20								
7. 19 42	17·83	2·09	0·12	240	0·15	244	0·04	202
8. 7 58								
8. 20 26	17·88	1·89	0·10	222	0·10	249	0·04	200
9. 8 47								
9. 21 48	17·58	1·70	0·30	301	0·16	319	0·09	239
10. 10 5								
10. 22 22	17·34	1·26	0·24	303	0·18	297	0·02	23
11. 11 9								
11. 23 55	17·51	0·86	0·21	316	0·13	354	0·05	151
12. 12 41								
13. 1 26	17·59	0·68	0·18	15	0·07	44	0·02	0
13. 14 40	17·54	0·77	0·15	96	0·05	55	0·09	322
14. 4 8								
14. 16 7	17·28	0·97	0·07	315	0·04	311	0·03	106
15. 4 45								
15. 17 20	17·16	0·98	0·11	28	0·04	296	0·06	323
16. 5 46								
16. 17 46	17·52	1·08	0·08	15	0·04	310	0·01	291
17. 6 0								
17. 18 29	17·50	1·12	0·05	358	0·05	347	0·03	273
18. 6 37								
18. 18 55	17·77	1·26	0·05	344	0·10	304	0·01	176
19. 6 55								
19. 19 9	17·98	1·27	0·09	342	0·09	293	0·03	338
20. 7 5								
20. 19 32	17·38	1·34	0·13	337	0·14	288	0·04	230
21. 7 48								
21. 20 20	17·05	1·27	0·13	316	0·09	286	0·04	230
22. 8 31								
22. 20 42	17·57	1·21	0·23	316	0·11	273	0·02	137
.....	....	....	....	....	....	....	....	....

Height of the Water in each individual tide at Glenarm, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 23. 23 50	18·64	2·75	0·06	74	0·05	205	0·04	318
24. 12 5								
25. 0 20	18·63	2·72	0·08	114	0·08	238	0·01	346
25. 12 38								
26. 0 56	18·15	2·72	0·05	42	0·08	237	0·05	41
26. 13 20								
27. 1 44	17·48	2·61	0·08	96	0·07	260	0·01	235
27. 14 5								
28. 2 26	17·87	2·51	0·12	151	0·03	261	0·03	344
28. 14 50								
29. 3 13	17·58	2·55	0·14	171	0·09	256	0·03	256
29. 15 27	17·32	2·42	0·04	241	0·09	230	0·04	355
30. 3 51								
30. 16 22	17·55	2·30	0·07	220	0·02	265	0·04	11
July 1. 4 47								
1. 17 0	17·59	2·19	0·18	332	0·14	243	0·06	144
2. 5 23								
2. 18 9	17·71	2·20	0·13	12	0·05	249	0·03	189
3. 6 42								
3. 19 15	18·47	2·28	0·12	29	0·08	241	0·01	146
4. 7 42								
4. 20 10	18·31	2·42	0·19	37	0·02	294	0·05	260
5. 8 43								
5. 21 9	17·34	2·65	0·18	72	0·06	186	0·04	282
6. 9 35								
6. 22 3	17·78	2·64	0·12	76	0·08	215	0·02	50
7. 10 19								
7. 22 36	17·99	2·88	0·15	54	0·06	200	0·03	219
8. 10 59								
8. 23 23	17·93	2·87	0·22	75	0·07	224	0·03	306
9. 11 46								
10. 0 9	18·11	2·96	0·24	76	0·15	243	0·05	280
10. 12 32								
11. 0 54	18·30	3·18	0·27	51	0·11	224	0·01	215
11. 13 19								
12. 1 43	17·87	3·03	0·25	81	0·10	229	0·06	314
12. 14 9								
13. 2 35	18·13	2·85	0·28	68	0·07	196	0·02	230
13. 14 52	17·62	3·00	0·22	85	0·05	139	0·03	328
14. 3 17								
14. 15 57	17·13	2·82	0·10	140	0·03	242	0·03	351
15. 4 25								
15. 16 58	17·47	2·62	0·05	257	0·06	243	0·03	76
16. 5 32								
16. 18 0	17·81	2·58	0·03	309	0·03	189	0·03	268
17. 6 27								
17. 19 19	17·64	2·40	0·15	18	0·05	222	0·02	146
18. 8 3								
18. 20 26	17·60	2·58	0·11	74	0·03	271	0·03	252
19. 8 52								
19. 21 6	17·45	2·57	0·09	55	0·07	254	0·03	93
20. 9 36								
20. 22 15	17·30	2·62	0·10	343	0·01	179	0·01	71
21. 10 32								
21. 22 49	17·28	2·70	0·08	38	0·02	243	0·03	75
22. 11 6								
22. 23 23	17·26	2·73	0·07	31	0·04	234	0·02	33
23. 11 41								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 24. 0 0	17·28	2·72	0·06	61	0·05	268	0·03	171
24. 12 17								
25. 0 34	17·34	2·69	0·12	80	0·06	238	0·01	0
25. 12 54								
26. 1 15	17·35	2·66	0·17	112	0·07	267	0·03	37
26. 13 32								
27. 1 49	17·16	2·62	0·12	127	0·07	243	0·04	10
27. 14 4								
28. 2 19	17·23	2·55	0·06	148	0·06	204	0·00	350
28. 14 45								
29. 3 10	16·97	2·34	0·15	131	0·08	279	0·01	110
29. 15 26 / 30. 3 46	17·07	2·30	0·15	212	0·04	189	0·03	65
30. 15 56 / 31. 4 29	17·34	2·25	0·12	250	0·04	271	0·03	101
31. 17 15 / August 1. 5 54	17·48	2·14	0·12	334	0·05	240	0·03	110
1. 18 32 / 2. 7 4	18·04	2·10	0·16	27	0·04	276	0·02	193
2. 19 42 / 3. 8 21	17·86	2·31	0·20	77	0·01	347	0·03	311
3. 20 54 / 4. 9 14	18·09	2·63	0·18	86	0·05	276	0·02	290
4. 21 37	18·34	2·72	0·13	50	0·07	178	0·01	288
5. 9 56								
5. 22 15	18·10	2·93	0·17	74	0·09	211	0·01	189
6. 10 35								
6. 22 55	18·32	3·14	0·20	61	0·03	211	0·02	206
7. 11 15								
7. 23 35	18·17	3·20	0·29	63	0·13	205	0·04	271
8. 12 0								
9. 0 25	18·19	3·12	0·33	74	0·09	226	0·03	321
9. 12 51								
10. 1 17	18·26	3·15	0·30	75	0·09	221	0·01	1
10. 13 44								
11. 2 12	18·07	2·94	0·30	105	0·13	230	0·03	232
11. 14 36 / 12. 2 58	18·06	3·10	0·13	136	0·07	266	0·02	72
12. 15 24 / 13. 3 54	17·89	2·73	0·13	216	0·11	232	0·02	83
13. 16 32 / 14. 5 7	17·54	2·55	0·03	214	0·04	237	0·02	106
14. 17 40 / 15. 6 25	17·45	2·12	0·06	329	0·02	307	0·01	97
15. 19 6 / 16. 7 43	17·35	2·22	0·09	39	0·04	208	0·02	88
16. 20 10 / 17. 8 40	17·74	2·32	0·07	64	0·02	181	0·04	169
17. 21 16	17·71	2·41	0·12	24	0·01	293	0·02	355
18. 9 34								
18. 21 51	17·95	2·68	0·09	16	0·09	205	0·04	48
19. 10 12								
19. 22 32	18·20	2·58	0·10	52	0·05	198	0·03	96
20. 10 48								
20. 23 4	17·69	2·70	0·09	116	0·07	227	0·01	247
21. 11 19								
21. 23 33	17·25	2·70	0·08	78	0·04	232	0·03	43
22. 11 49								
23. 0 4	17·76	2·72	0·13	109	0·04	174	0·07	294
.....	....	....	....	....	....	....	....	....

Height of the Water in each individual tide at Donaghadee, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.		ft.		ft.	
June 22. 23 23	18·22	4·78	0·14	72°	0·07	161°	0·01	221°
23. 11 45								
24. 0 7	18·70	4·90	0·06	325	0·03	221	0·02	61
24. 12 25								
25. 0 43	18·78	4·86	0·06	16	0·06	245	0·03	303
25. 13 2								
26. 1 21	18·34	4·76	0·06	356	0·09	240	0·07	31
26. 13 42								
27. 2 2	17·56	4·51	0·09	246	0·04	251	0·05	65
27. 14 20								
28. 2 37	18·01	4·29	0·01	260	0·04	211	0·03	322
28. 14 59								
29. 3 20	17·81	3·97	0·08	200	0·04	208	0·06	31
29. 15 40 / 30. 4 4	17·69	3·77	0·04	238	0·06	241	0·03	94
30. 16 25 / July 1. 4 50	17·85	3·56	0·08	264	0·03	201	0·01	218
1. 17 25 / 2. 5 48	17·93	3·30	0·07	308	0·05	222	0·01	251
2. 18 22 / 3. 6 55	18·00	3·36	0·09	12	0·04	215	0·01	240
3. 19 31 / 4. 7 58	18·70	3·58	0·10	6	0·04	263	0·03	349
4. 20 25 / 5. 8 58	18·45	3·90	0·12	24	0·05	277	0·03	258
5. 21 23 / 6. 9 49	17·58	4·40	0·15	42	0·07	269	0·04	126
6. 22 13	18·01	4·61	0·09	60	0·01	53	0·05	208
7. 10 32								
7. 22 52	18·21	5·12	0·08	40	0·04	172	0·02	297
8. 11 14								
8. 23 35	18·00	5·36	0·07	66	0·04	162	0·04	43
9. 11 59								
10. 0 22	18·18	5·54	0·05	101	0·10	212	0·03	271
10. 12 45								
11. 1 7	18·37	5·71	0·11	17	0·06	199	0·06	247
11. 13 31								
12. 1 55	17·93	5·49	0·15	66	0·05	193	0·06	331
12. 14 21								
13. 2 47	18·16	5·22	0·09	34	0·07	175	0·03	317
13. 15 13 / 14. 3 38	17·59	5·06	0·07	182	0·05	236	0·04	57
14. 16 7 / 15. 4 35	17·28	4·60	0·06	204	0·03	216	0·02	9
15. 17 10 / 16. 5 44	17·69	4·15	0·07	290	0·05	241	0·02	49
16. 18 21 / 17. 6 48	17·96	3·88	0·11	337	0·11	230	0·06	56
17. 19 29 / 18. 8 13	17·93	3·85	0·11	13	0·02	312	0·03	198
18. 20 39 / 19. 9 5	17·74	4·01	0·08	71	0·01	257	0·01	236
19. 21 35	17·66	4·30	0·07	333	0·01	41	0·03	11
20. 9 59								
20. 22 23	17·64	4·53	0·07	334	0·01	250	0·04	41
21. 10 42								
21. 23 1	17·58	4·80	0·04	328	0·01	132	0·02	159
22. 11 19								
22. 23 37	17·50	4·90	0·07	328	0·03	123	0·00	31
23. 11 55								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 24. 0 12	17·54	4·91	0·03	11	0·02	163	0·03	136
24. 12 29								
25. 0 46	17·55	4·87	0·03	104	0·05	140	0·01	151
25. 13 2								
26. 1 18	17·57	4·71	0·06	110	0·01	214	0·03	61
26. 13 36								
27. 1 54	17·43	4·56	0·02	157	0·02	318	0·04	314
27. 14 12								
28. 2 31	17·58	4·31	0·08	118	0·02	212	0·02	74
28. 14 49 / 29. 3 9	17·28	4·08	0·09	206	0·08	233	0·05	71
29. 15 26 / 30. 3 46	17·39	3·72	0·08	251	0·08	246	0·04	112
30. 16 18 / 31. 4 51	17·39	3·40	0·09	261	0·04	251	0·01	111
31. 17 22 / August 1. 6 1	17·55	3·24	0·11	314	0·02	179	0·03	124
1. 18 45 / 2. 7 17	17·97	3·26	0·12	9	0·03	211	0·03	147
2. 19 58 / 3. 8 37	17·78	3·69	0·12	60	0·08	69	0·02	325
3. 21 8	17·98	4·11	0·03	122	0·04	164	0·00	179
4. 9 37								
4. 22 7	18·24	4·81	0·06	6	0·04	158	0·01	117
5. 10 23								
5. 22 39	17·99	5·28	0·09	99	0·02	162	0·07	321
6. 11 0								
6. 23 22	18·18	5·72	0·13	52	0·05	174	0·02	104
7. 11 44								
8. 0 5	17·88	5·98	0·10	68	0·06	152	0·05	319
8. 12 29								
9. 0 52	18·02	6·01	0·15	67	0·07	174	0·05	3
9. 13 14								
10. 1 36	18·03	5·89	0·11	51	0·06	166	0·01	135
10. 14 1								
11. 2 25	17·93	5·46	0·14	92	0·05	196	0·06	9
11. 14 47 / 12. 3 9	17·93	5·20	0·11	221	0·03	237	0·03	28
12. 15 40 / 13. 4 8	17·76	4·50	0·16	224	0·03	223	0·02	82
13. 16 42 / 14. 5 17	17·50	3·92	0·06	234	0·03	232	0·03	108
14. 18 0 / 15. 6 45	17·46	3·45	0·08	277	0·01	99	0·02	41
15. 19 22 / 16. 7 59	17·35	3·42	0·05	6	0·03	286	0·03	192
16. 20 33 / 17. 9 3	17·81	3·77	0·06	4	0·04	218	0·01	138
17. 21 29	17·74	4·02	0·05	347	0·03	164	0·01	43
18. 9 51								
18. 22 12	17·98	4·51	0·07	344	0·06	168	0·05	71
19. 10 31								
19. 22 49	18·12	4·54	0·09	25	0·09	81	0·06	36
20. 11 5								
20. 23 21	17·65	4·90	0·01	141	0·05	202	0·03	341
21. 11 37								
21. 23 52	17·25	4·95	0·04	30	0·06	179	0·04	182
22. 12 6								
23. 0 20	17·73	5·02	0·06	129	0·07	164	0·04	164
.....	....	....	....	....	....	....	....	....

Height of the Water in each individual tide at Ardglass, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 22. 23 30	18·35	6·30	0·20	87	0·12	175	0·04	211
23. 11 45								
24. 0 0	18·60	6·35	0·17	73	0·12	231	0·09	188
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
26. 1 13	18·20	6·11	0·15	110	0·05	216	0·03	105
26. 13 34								
27. 1 55	17·62	5·76	0·10	96	0·07	146	0·04	28
27. 14 14								
28. 2 33	17·83	5·40	0·10	99	0·06	188	0·02	61
28. 14 54								
29. 3 15	17·76	4·99	0·14	125	0·05	170	0·01	312
29. 15 36 / 30. 4 0	17·59	4·71	0·09	131	0·11	217	0·02	176
30. 16 26 / July 1. 4 51	17·71	4·38	0·09	173	0·06	196	0·02	195
1. 17 22 / 2. 5 45	17·77	4·02	0·06	233	0·07	238	0·01	267
2. 18 22 / 3. 6 55	17·81	4·06	0·01	42	0·04	240	0·02	224
3. 19 33 / 4. 8 0	18·49	4·33	0·07	345	0·10	244	0·02	43
4. 20 27 / 5. 9 0	18·22	4·81	0·24	71	0·15	301	0·14	176
5. 21 22 / 6. 9 48	17·41	5·60	0·12	77	0·06	222	0·05	146
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
7. 22 56	18·23	6·68	0·12	138	0·11	5	0·08	190
8. 11 16								
8. 23 35	17·76	7·08	0·19	90	0·05	47	0·00	208
9. 11 56								
10. 0 17	17·90	7·24	0·18	93	0·09	157	0·09	35
10. 12 41								
11. 1 5	18·29	7·62	0·15	64	0·16	196	0·16	145
11. 13 30								
12. 1 54	17·75	7·30	0·22	100	0·01	106	0·01	270
12. 14 18								
13. 2 43	17·87	6·78	0·15	69	0·08	137	0·05	109
13. 15 11 / 14. 3 36	17·54	6·53	0·16	116	0·04	128	0·05	215
14. 16 9 / 15. 4 37	17·24	5·81	0·15	121	0·02	234	0·03	254
15. 17 13 / 16. 5 47	17·52	5·10	0·05	138	0·05	246	0·02	81
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
17. 19 40 / 18. 8 24	17·77	4·88	0·07	356	0·08	250	0·06	179
18. 20 52 / 19. 9 18	17·74	5·26	0·07	53	0·04	232	0·06	171
19. 21 47	17·64	5·50	0·08	60	0·04	296	0·06	124
20. 10 11								
20. 22 35	17·60	5·90	0·07	118	0·08	189	0·03	116
21. 10 54								
21. 23 12	17·54	6·18	0·11	89	0·06	187	0·04	254
22. 11 29								
22. 23 46	17·46	6·39	0·11	132	0·06	143	0·01	112
23. 12 3								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 24. 0 20	17·48	6·40	0·10	140	0·08	144	0·03	199
24. 12 38								
25. 0 55	17·48	6·34	0·16	99	0·03	174	0·03	26
25. 13 9								
26. 1 24	17·52	6·18	0·15	118	0·05	144	0·07	196
26. 13 41								
27. 1 58	17·40	5·85	0·21	118	0·07	148	0·02	200
27. 14 15								
28. 2 33	17·47	5·48	0·20	117	0·03	123	0·03	157
28. 14 53 / 29. 3 13	17·29	5·07	0·16	129	0·09	187	0·06	213
29. 15 33 / 30. 3 53	17·35	4·67	0·12	148	0·04	221	0·03	155
30. 16 21 / 31. 4 54	17·34	4·15	0·12	156	0·01	257	0·05	170
31. 17 28 / August 1. 6 7	17·38	3·88	0·07	198	0·02	255	0·01	77
1. 18 48 / 2. 7 20	17·71	3·98	0·09	351	0·05	234	0·03	143
2. 20 2 / 3. 8 41	17·58	4·63	0·09	63	0·03	55	0·02	197
3. 21 8	17·85	5·25	0·13	105	0·04	168	0·05	209
4. 9 32								
4. 21 55	18·03	6·21	0·12	137	0·08	132	0·04	165
5. 10 18								
5. 22 42	17·91	6·99	0·24	99	0·02	140	0·06	146
6. 11 1								
6. 23 20	18·05	7·60	0·26	78	0·03	124	0·09	127
7. 11 42								
8. 0 3	17·83	8·11	0·27	89	0·12	109	0·04	327
8. 12 25								
9. 0 47	17·88	8·12	0·25	95	0·07	138	0·09	79
9. 13 9								
10. 1 32	17·85	7·80	0·20	87	0·07	139	0·08	57
10. 13 56								
11. 2 20	17·81	7·13	0·32	91	0·04	88	0·03	68
11. 14 51 / 12. 3 13	17·88	6·64	0·18	133	0·08	173	0·07	73
12. 15 46 / 13. 4 14	17·70	5·62	0·14	132	0·06	193	0·10	127
13. 16 51 / 14. 5 26	17·42	4·81	0·10	139	0·03	186	0·01	158
14. 18 7 / 15. 6 52	17·36	4·22	0·04	172	0·02	313	0·01	253
15. 19 38 / 16. 8 15	17·30	4·29	0·06	95	0·02	84	0·02	182
16. 20 46 / 17. 9 16	17·71	4·75	0·13	88	0·03	253	0·04	135
17. 21 40	17·66	5·20	0·08	140	0·02	174	0·02	149
18. 10 1								
18. 22 21	17·91	5·80	0·13	76	0·16	211	0·10	99
19. 10 36								
19. 22 51	17·99	6·00	0·24	62	0·05	204	0·03	92
20. 11 5								
20. 23 18	17·57	6·45	0·27	146	0·10	236	0·09	340
21. 11 37								
21. 23 55	17·15	6·53	0·20	117	0·05	166	0·03	27
22. 12 7								
23. 0 19	17·62	6·57	0·14	111	0·06	111	0·05	210
.....	....	....	....	....	....	....	....	....

Height of the Water in each individual tide at Clogher Head, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 24. 0 6	17·96	6·22	0·22	126	0·07	161	0·05	200
24. 12 24								
25. 0 42	17·93	6·16	0·25	113	0·06	166	0·03	307
25. 12 59								
26. 1 16	17·48	5·99	0·23	124	0·04	202	0·05	167
26. 13 37								
27. 1 57	17·15	5·58	0·27	116	0·07	145	0·02	162
27. 14 17								
28. 2 36	17·35	5·30	0·16	119	0·05	120	0·04	177
28. 14 57								
29. 3 18	17·29	4·79	0·21	123	0·05	153	0·03	233
29. 15 39 / 30. 4 3	17·13	4·50	0·19	137	0·02	85	0·08	178
30. 16 27 / July 1. 4 52	17·26	4·18	0·13	149	0·10	190	0·03	116
1. 17 27 / 2. 5 50	17·21	3·83	0·18	178	0·10	281	0·05	156
2. 18 29 / 3. 7 2	17·34	3·99	0·08	175	0·08	212	0·03	68
3. 19 32 / 4. 7 59	17·88	4·28	0·06	336	0·10	227	0·07	205
4. 20 31 / 5. 9 4	17·52	4·90	0·10	141	0·06	52	0·04	126
5. 21 28	17·01	5·28	0·20	117	0·03	184	0·03	207
6. 9 53								
6. 22 17	17·40	6·00	0·21	113	0·11	145	0·10	106
7. 10 37								
7. 22 57	17·61	6·61	0·24	119	0·08	118	0·04	113
8. 11 21								
8. 23 44	17·53	7·11	0·41	111	0·05	101	0·05	325
9. 12 7								
10. 0 29	17·54	7·36	0·34	111	0·05	75	0·02	85
10. 12 54								
11. 1 19	17·95	7·50	0·29	123	0·08	203	0·10	253
11. 13 43								
12. 2 6	17·29	7·27	0·40	124	0·06	202	0·05	274
12. 14 31								
13. 2 55	17·34	6·71	0·25	103	0·04	170	0·08	194
13. 15 15 / 14. 3 40	16·72	6·16	0·07	291	0·23	124	0·07	209
14. 16 24 / 15. 4 52	16·84	5·68	0·24	133	0·02	185	0·04	254
15. 17 28 / 16. 6 2	17·14	5·10	0·14	113	0·09	226	0·03	45
16. 18 32 / 17. 7 0	17·10	4·95	0·13	205	0·14	204	0·08	329
17. 19 44 / 18. 8 28	17·30	4·60	0·11	288	0·16	159	0·11	336
18. 21 6 / 19. 9 32	17·14	5·20	0·11	195	0·07	252	0·07	187
19. 21 59	17·26	5·20	0·11	339	0·13	170	0·10	274
20. 10 23								
20. 22 46	17·15	5·61	0·18	121	0·09	306	0·05	122
21. 11 3								
21. 23 19	17·10	6·04	0·22	125	0·06	146	0·04	291
22. 11 37								
22. 23 54	17·11	6·02	0·19	76	0·25	165	0·14	310
23. 12 13								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 24. 0 32	16·97	6·07	0·04	17	0·28	176	0·08	20
24. 12 51								
25. 1 10	17·24	5·99	0·25	107	0·38	180	0·14	251
25. 13 20								
26. 1 29	16·99	6·05	0·06	143	0·16	143	0·05	276
26. 13 46								
27. 2 2	16·94	5·70	0·36	121	0·16	214	0·05	130
27. 14 20								
28. 2 37	17·11	5·24	0·27	106	0·02	239	0·06	228
28. 15 5 / 29. 3 25	17·02	5·01	0·28	96	0·16	162	0·05	244
29. 15 45 / 30. 4 5	16·94	4·60	0·27	150	0·12	77	0·06	261
30. 16 41 / 31. 5 14	16·81	3·96	0·18	227	0·12	151	0·10	238
31. 17 38 / August 1. 6 17	16·99	3·68	0·14	203	0·13	195	0·05	280
1. 18 59 / 2. 7 31	17·29	3·85	0·06	295	0·07	234	0·03	193
2. 20 12 / 3. 8 51	17·38	4·29	0·12	128	0·04	167	0·01	174
3. 21 16 / 4. 9 35	17·49	5·23	0·08	125	0·05	186	0·04	133
4. 22 17	16·95	6·06	0·52	230	0·15	236	0·08	106
5. 10 33								
5. 22 48	17·44	6·99	0·24	108	0·08	70	0·06	130
6. 11 6								
6. 23 24	17·53	7·78	0·30	102	0·06	72	0·05	135
7. 11 48								
8. 0 12	17·16	8·03	0·31	123	0·07	153	0·10	178
8. 12 35								
9. 0 57	17·18	8·10	0·03	113	0·17	305	0·10	345
9. 13 16								
10. 1 34	17·37	7·76	0·34	109	0·11	85	0·10	113
10. 14 2								
11. 2 30	17·54	6·91	0·63	87	0·19	185	0·10	327
11. 14 48 / 12. 3 10	17·26	6·39	0·32	140	0·24	103	0·09	186
12. 15 48 / 13. 4 16	17·27	5·34	0·28	88	0·19	148	0·10	339
13. 16 56 / 14. 5 31	17·36	4·64	0·57	103	0·14	227	0·03	41
14. 18 6 / 15. 6 51	17·16	3·99	0·35	122	0·14	135	0·05	81
15. 19 46 / 16. 8 23	17·11	4·21	0·28	71	0·10	253	0·05	27
16. 20 53 / 17. 9 23	17·48	4·66	0·26	112	0·12	350	0·06	11
17. 21 50	17·23	4·97	0·19	142	0·03	177	0·05	266
18. 10 11								
18. 22 32	17·50	5·70	0·18	96	0·04	121	0·12	45
19. 10 45								
19. 22 57	17·70	5·92	0·43	100	0·10	183	0·08	56
20. 11 11								
20. 23 24	16·85	6·20	0·19	324	0·19	178	0·04	245
21. 11 42								
22. 0 0	16·76	6·27	0·22	108	0·26	171	0·07	296
22. 12 15								
23. 0 29	17·16	6·59	0·18	124	0·09	325	0·02	238
.....	....	....	....	....	....	....	....	....

Height of the Water in each individual tide at Kingstown, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 22. 23 21	17·99	4·91	0·33	147	0·06	258	0·06	165
23. 11 52								
24. 0 11	18·14	4·70	0·35	149	0·03	39	0·01	31
24. 12 28								
25. 0 45	18·12	4·69	0·31	147	0·02	129	0·05	132
25. 13 3								
26. 1 21	17·72	4·56	0·38	153	0·08	351	0·03	72
26. 13 43								
27. 2 4	17·35	4·19	0·33	146	0·02	253	0·03	197
27. 14 28	17·24	4·14	0·26	152	0·05	238	0·02	33
28. 2 40								
28. 15 0	17·42	3·84	0·27	154	0·04	153	0·04	141
29. 3 29								
29. 15 50	17·31	3·48	0·25	83	0·06	144	0·04	8
30. 4 15								
30. 16 45	17·48	3·18	0·23	178	0·03	189	0·02	89
July 1. 5 10								
1. 17 47	17·50	2·95	0·25	177	0·06	202	0·04	200
2. 6 10								
2. 18 44	17·60	3·04	0·17	196	0·02	273	0·00	264
3. 7 17								
3. 19 51	18·18	3·21	0·18	195	0·04	216	0·01	40
4. 8 18								
4. 20 43	17·86	3·65	0·16	182	0·01	148	0·03	184
5. 9 16								
5. 21 39	17·18	4·19	0·25	171	0·02	17	0·01	4
6. 10 5								
6. 22 20	17·55	4·66	0·31	159	0·06	70	0·04	158
7. 10 39								
7. 22 57	17·87	5·13	0·35	151	0·06	33	0·03	164
8. 11 21								
8. 23 45	17·53	5·61	0·43	147	0·05	304	0·02	115
9. 12 6								
10. 0 27	17·69	5·68	0·50	134	0·06	314	0·04	104
10. 12 54								
11. 1 20	18·04	5·88	0·36	146	0·09	47	0·04	113
11. 13 43								
12. 2 5	17·37	5·52	0·44	138	0·03	340	0·02	342
12. 14 32								
13. 2 58	17·40	5·13	0·33	143	0·05	55	0·03	160
13. 15 31	17·19	4·88	0·43	143	0·04	17	0·04	229
14. 3 56								
14. 16 33	17·07	4·35	0·38	155	0·02	334	0·02	289
15. 5 1								
15. 17 38	17·50	3·86	0·30	164	0·02	229	0·02	196
16. 6 12								
16. 18 51	17·70	3·66	0·27	180	0·01	312	0·03	128
17. 7 21								
17. 19 57	17·70	3·63	0·27	175	0·01	337	0·03	190
18. 8 41								
18. 21 9	17·53	3·76	0·28	174	0·03	122	0·04	151
19. 9 37								
19. 22 5	17·58	4·08	0·28	165	0·05	214	0·05	348
20. 10 25								
20. 22 45	17·34	4·32	0·35	165	0·01	211	0·02	48
21. 11 4								
21. 23 23	17·34	4·59	0·37	152	0·05	269	0·03	140
22. 11 39								
22. 23 54	17·22	4·76	0·34	149	0·02	39	0·02	53
23. 12 10								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 24. 0 26	17·27	4·74	0·33	145	0·05	85	0·04	159
24. 12 42								
25. 0 58	17·17	4·66	0·36	142	0·06	8	0·05	91
25. 13 14								
26. 1 30	17·27	4·48	0·29	148	0·03	88	0·05	229
26. 13 48								
27. 2 6	17·23	4·28	0·32	141	0·05	257	0·04	23
27. 14 24								
28. 2 42	17·24	3·98	0·25	144	0·03	177	0·02	7
28. 15 3 / 29. 3 23	17·16	3·71	0·18	150	0·03	328	0·03	116
29. 15 47 / 30. 4 7	17·17	3·39	0·25	160	0·03	169	0·04	142
30. 16 41 / 31. 5 14	17·12	3·01	0·22	160	0·04	279	0·06	141
31. 17 48 / August 1. 6 27	17·19	2·86	0·27	189	0·01	108	0·02	70
1. 19 8 / 2. 7 40	17·48	3·08	0·11	214	0·05	214	0·03	65
2. 20 23 / 3. 9 2	17·33	3·50	0·15	175	0·04	211	0·02	185
3. 21 20	17·52	3·91	0·19	147	0·03	101	0·02	168
4. 9 42								
4. 22 3	17·75	4·73	0·34	141	0·03	168	0·00	187
5. 10 26								
5. 22 48	17·59	5·39	0·34	148	0·04	40	0·01	174
6. 11 8								
6. 23 28	17·70	5·91	0·44	135	0·07	14	0·10	133
7. 11 47								
8. 0 6	17·38	6·39	0·49	130	0·07	336	0·07	90
8. 12 29								
9. 0 51	17·62	6·31	0·50	124	0·01	309	0·08	108
9. 13 15								
10. 1 40	17·61	6·05	0·33	141	0·07	6	0·05	47
10. 14 3								
11. 2 26	17·50	5·47	0·46	133	0·02	320	0·05	179
11. 15 7 / 12. 3 29	17·56	4·91	0·46	154	0·08	22	0·10	212
12. 15 55 / 13. 4 23	17·36	4·17	0·21	141	0·09	230	0·06	128
13. 17 8 / 14. 5 43	17·17	3·58	0·26	168	0·02	343	0·02	152
14. 18 28 / 15. 7 13	17·22	3·24	0·19	178	0·05	274	0·04	182
15. 19 54 / 16. 8 31	17·13	3·25	0·19	185	0·02	215	0·02	157
16. 20 57 / 17. 9 27	17·63	3·58	0·20	157	0·04	217	0·01	127
17. 21 55	17·49	3·69	0·27	193	0·09	255	0·01	46
18. 10 12								
18. 22 28	17·67	4·30	0·25	165	0·00	357	0·01	8
19. 10 41								
19. 22 53	17·64	4·49	0·34	157	0·05	24	0·04	210
20. 11 9								
20. 23 25	17·37	4·81	0·34	144	0·01	269	0·01	314
21. 11 44								
22. 0 3	16·84	4·94	0·28	147	0·05	308	0·06	103
22. 12 15								
23. 0 26	17·42	4·92	0·31	143	0·05	57	0·03	104
.....	....	....	....	....	....	....	....	....

Height of the Water in each individual tide at Dunmore East, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 22. 17 37	16·29	5·30	0·23	16	0·07	82	0·04	239
23. 5 55								
23. 18 21	16·38	5·32	0·07	346	0·15	115	0·06	232
24. 6 45								
24. 18 53	16·23	5·12	0·13	321	0·14	126	0·03	263
25. 7 12	16·37	5·07	0·06	306	0·17	146	0·08	166
25. 19 31	16·25	5·16	0·11	327	0·12	89	0·03	314
26. 7 47	16·02	4·98	0·13	2	0·14	99	0·06	215
26. 20 5	15·71	4·98	0·10	340	0·17	107	0·01	158
27. 8 22	15·75	4·88	0·14	5	0·14	75	0·06	273
27. 20 36	15·69	4·62	0·07	49	0·18	97	0·05	250
28. 8 52	15·73	4·43	0·07	11	0·20	74	0·03	206
28. 21 17	15·84	4·28	0·06	17	0·17	89	0·02	320
29. 9 33	15·74	4·07	0·07	322	0·18	95	0·06	250
29. 21 50	15·88	3·98	0·08	9	0·14	80	0·02	207
30. 10 14	15·90	3·80	0·09	356	0·13	102	0·07	165
30. 22 24	15·84	3·56	0·10	341	0·07	113	0·03	267
July 1. 10 57	15·85	3·40	0·09	347	0·08	134	0·03	67
1. 23 22	15·81	3·19	0·12	345	0·08	129	0·01	142
2. 11 51	15·76	3·13	0·04	336	0·08	142	0·03	259
3. 0 39	16·28	3·08	0·18	13	0·08	115	0·02	131
3. 13 17								
4. 1 54	16·63	3·50	0·10	28	0·09	153	0·03	213
4. 14 26								
5. 2 57	16·04	3·93	0·07	67	0·02	190	0·04	194
5. 15 20								
6. 3 43	15·58	4·52	0·09	360	0·09	114	0·03	244
....	....	....	....	....	....	....	....	....
....	....	....	....	....	....	....	....	....
7. 17 3	16·06	5·38	0·15	44	0·16	129	0·04	25
8. 5 21								
8. 17 49	15·93	5·81	0·20	3	0·15	106	0·02	268
9. 6 9								
9. 18 30	16·00	6·21	0·12	15	0·18	90	0·02	224
10. 6 54								
10. 19 12	16·41	6·21	0·26	10	0·18	105	0·06	208
11. 7 36								
11. 20 2	15·73	6·00	0·15	16	0·22	97	0·01	32
12. 8 25								
12. 20 48	15·64	5·69	0·12	15	0·24	105	0·05	242
13. 9 12								
13. 21 36	15·36	5·08	0·17	339	0·14	114	0·02	134
14. 10 4								
14. 22 31	15·51	4·52	0·18	6	0·14	114	0·05	294
15. 10 56								
15. 23 22	16·02	3·92	0·21	2	0·12	115	0·04	338
16. 11 58								
17. 0 34	16·20	3·55	0·19	3	0·07	136	0·03	262
17. 13 15								
18. 1 55	16·11	3·62	0·10	48	0·12	151	0·03	141
18. 14 29								
19. 3 3	16·05	4·10	0·08	36	0·08	88	0·01	49
19. 15 33	16·05	4·45	0·15	15	0·09	127	0·01	119
20. 4 0								
20. 16 25	15·78	4·80	0·21	6	0·11	118	0·03	165
21. 4 43								
21. 17 15	15·83	5·13	0·19	27	0·12	109	0·03	97
22. 5 31								
22. 17 53	15·73	5·31	0·15	39	0·17	91	0·02	272
23. 6 10								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 18 22 / 24. 6 38	15·72	5·39	0·18	13	0·14	96	0·08	141
24. 18 55 / 25. 7 15	15·82	5·35	0·13	17	0·19	76	0·05	244
25. 19 30 / 26. 7 47	15·77	5·21	0·19	31	0·21	47	0·08	316
26. 20 2 / 27. 8 18	15·65	4·90	0·12	339	0·15	95	0·05	284
27. 20 33	15·57	4·62	0·14	8	0·18	91	0·03	213
28. 8 49								
28. 21 5	15·54	4·22	0·20	322	0·21	93	0·02	254
29. 9 22								
29. 21 39	15·66	3·86	0·10	352	0·13	84	0·05	191
30. 10 4								
30. 22 28	15·51	3·33	0·18	328	0·09	116	0·04	96
31. 11 2								
31. 23 36	15·64	2·92	0·11	3	0·01	125	0·02	285
August 1. 12 22								
2. 1 7	15·89	2·90	0·13	7	0·05	196	0·03	147
2. 13 50								
3. 2 33	15·72	3·55	0·08	107	0·04	143	0·06	269
3. 15 7 / 4. 3 39	15·69	4·29	0·08	50	0·11	109	0·04	52
4. 16 13 / 5. 4 33	15·87	5·00	0·12	23	0·20	105	0·08	347
5. 16 57 / 6. 5 21	15·91	5·90	0·17	10	0·12	95	0·08	176
6. 17 49 / 7. 6 9	15·93	6·40	0·14	22	0·16	100	0·01	133
7. 18 34 / 8. 6 57	15·86	6·60	0·10	343	0·20	68	0·10	40
8. 19 20 / 9. 7 41	15·99	6·68	0·09	6	0·23	80	0·11	5
9. 20 4	16·00	6·51	0·10	346	0·21	108	0·01	37
10. 8 24								
10. 20 44	15·68	5·92	0·14	358	0·27	107	0·04	29
11. 9 4								
11. 21 24	15·72	5·33	0·22	5	0·22	93	0·02	140
12. 9 47								
12. 22 10	15·53	4·46	0·02	6	0·23	101	0·02	322
13. 10 37								
13. 23 3	15·39	3·64	0·18	329	0·13	128	0·01	50
14. 11 42								
15. 0 21	15·56	3·06	0·14	10	0·08	137	0·01	84
15. 13 6								
16. 1 51	15·57	3·23	0·09	14	0·07	167	0·02	131
16. 14 30								
17. 3 9	16·00	3·80	0·10	17	0·08	108	0·03	158
17. 15 30 / 18. 4 0	16·03	4·31	0·22	0	0·12	127	0·05	120
18. 16 22 / 19. 4 43	16·15	4·85	0·06	71	0·23	57	0·10	235
19. 17 5 / 20. 5 19	15·94	5·03	0·10	353	0·12	120	0·04	219
20. 17 40 / 21. 5 56	15·77	5·38	0·08	347	0·15	95	0·07	203
21. 18 16 / 22. 6 28	15·46	5·60	0·14	356	0·18	84	0·03	257
22. 18 41 / 23. 6 59	16·00	5·59	0·06	26	0·23	80	0·03	326

Height of the Water in each individual tide at New Ross, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
June 22. 18 14	17·32	5·88	0·05	350	0·31	93	0·01	259
23. 6 36								
23. 18 55	17·47	5·85	0·40	350	0·34	92	0·17	140
24. 7 23								
24. 19 44	17·47	5·65	0·29	340	0·40	108	0·04	107
25. 7 54								
25. 20 13	16·80	5·61	0·61	342	0·22	100	0·05	104
26. 8 27								
26. 20 51	16·57	5·43	0·43	334	0·32	101	0·02	58
27. 9 6								
27. 21 21	16·61	5·18	0·37	334	0·27	91	0·01	322
28. 9 38								
28. 21 54	16·57	4·83	0·32	329	0·23	87	0·03	300
29. 10 14								
29. 22 33	16·53	4·50	0·37	343	0·25	89	0·11	114
30. 10 52								
30. 23 10	16·55	4·25	0·27	328	0·15	85	0·03	10
July 1. 11 39								
2. 0 7	16·50	3·75	0·30	331	0·17	89	0·02	69
2. 12 42								
3. 1 18	16·96	3·69	0·26	354	0·19	91	0·01	222
3. 13 54								
4. 2 29	17·56	3·96	0·13	327	0·13	104	0·05	25
4. 14 54	17·19	4·34	0·25	350	0·17	99	0·03	158
5. 3 35								
5. 16 14	16·30	5·00	0·33	355	0·23	107	0·05	137
6. 4 45								
6. 17 14	16·94	5·48	0·44	355	0·28	99	0·07	109
7. 5 41								
7. 18 3	17·11	5·88	0·51	341	0·39	97	0·03	106
8. 6 21								
8. 18 58	16·86	6·22	0·52	339	0·36	109	0·04	116
9. 7 18								
9. 19 49	17·03	6·43	0·53	346	0·38	104	0·07	131
10. 8 8								
10. 20 28	17·74	6·39	0·51	343	0·37	93	0·06	108
11. 8 54								
11. 21 19	17·04	6·34	0·66	344	0·35	99	0·06	56
12. 9 43								
12. 22 7	16·97	6·13	0·56	348	0·38	92	0·09	97
13. 10 29								
13. 22 48	16·39	5·67	0·49	341	0·30	95	0·03	104
14. 11 12								
14. 23 36	16·23	5·18	0·46	336	0·24	100	0·02	63
15. 12 2								
16. 0 27	16·81	4·67	0·39	343	0·26	96	0·01	327
16. 12 55								
17. 1 23	16·98	4·24	0·31	342	0·21	123	0·02	312
17. 14 6								
18. 2 49	16·92	4·25	0·30	358	0·18	103	0·01	246
18. 15 27	16·79	4·61	0·42	347	0·16	100	0·00	270
19. 4 0								
19. 16 37	16·79	5·08	0·40	352	0·21	104	0·02	352
20. 5 4								
20. 17 33	16·64	5·42	0·46	347	0·25	101	0·01	116
21. 5 51								
21. 18 14	16·69	5·75	0·52	347	0·29	91	0·03	68
22. 6 30								
22. 18 53	16·64	5·87	0·50	351	0·34	101	0·06	116
23. 7 10								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 19 27	16·59	5·93	0·51	351	0·31	93	0·10	109
24. 7 43								
24. 19 58	16·59	5·92	0·54	351	0·35	89	0·07	98
25. 8 18								
25. 20 31	16·59	5·80	0·47	343	0·34	91	0·04	47
26. 8 46								
26. 21 0	16·48	5·52	0·39	347	0·31	95	0·06	134
27. 9 15								
27. 21 31	16·36	5·25	0·40	346	0·25	92	0·01	339
28. 9 45								
28. 21 58	16·20	4·87	0·27	337	0·29	87	0·02	276
29. 10 15								
29. 22 32	16·24	4·49	0·39	328	0·23	65	0·03	23
30. 10 51								
30. 23 10	16·16	3·95	0·29	321	0·18	82	0·02	55
31. 11 41								
August 1. 0 12	16·35	3·58	0·32	329	0·13	90	0·01	242
1. 12 54								
2. 1 36	16·66	3·47	0·27	14	0·15	104	0·06	159
2. 14 23								
3. 3 10	16·40	4·17	0·20	6	0·13	94	0·05	227
3. 15 42	16·50	4·90	0·30	352	0·21	97	0·02	70
4. 4 21								
4. 16 54	16·82	5·61	0·43	341	0·35	85	0·02	314
5. 5 14								
5. 17 57	16·78	6·30	0·59	348	0·35	101	0·07	101
6. 6 21								
6. 18 47	17·05	6·76	0·58	351	0·39	100	0·07	90
7. 7 7								
7. 19 34	17·15	6·89	0·59	353	0·41	91	0·05	79
8. 7 57								
8. 20 27	17·13	6·95	0·68	348	0·42	98	0·12	92
9. 8 43								
9. 20 59	17·14	6·81	0·63	349	0·49	93	0·16	113
10. 9 21								
10. 21 42	16·77	6·31	0·59	342	0·39	106	0·09	85
11. 9 58								
11. 22 15	16·88	5·86	0·49	340	0·39	101	0·06	132
12. 10 36								
12. 22 57	16·33	5·09	0·41	336	0·26	99	0·03	66
13. 11 22								
13. 23 46	16·10	4·26	0·37	323	0·17	103	0·05	34
14. 12 22								
15. 0 58	16·26	3·70	0·25	343	0·17	105	0·05	301
15. 13 44								
16. 2 30	16·28	3·82	0·24	347	0·14	100	0·01	122
16. 15 14	16·52	4·20	0·27	343	0·13	122	0·04	43
17. 3 44								
17. 16 15	16·63	4·91	0·35	358	0·23	80	0·09	141
18. 4 47								
18. 17 11	16·87	5·39	0·38	348	0·28	95	0·05	164
19. 5 32								
19. 17 57	16·83	5·52	0·39	339	0·26	108	0·04	185
20. 6 11								
20. 18 35	16·59	5·96	0·56	347	0·35	93	0·08	69
21. 6 51								
21. 19 12	16·18	6·03	0·50	344	0·39	97	0·08	110
22. 7 24								
22. 19 33	16·84	6·09	0·48	352	0·34	92	0·01	144
23. 7 51								

Height of the Water in each individual tide at Passage West, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
June 24. 19 6 / 25. 7 16	16·13	5·37	0·36	279	0·06	104	0·03	92
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
26. 20 6 / 27. 8 25	15·52	5·02	0·31	290	0·12	80	0·03	68
27. 20 43 / 28. 9 3	15·55	4·64	0·32	289	0·11	60	0·03	122
28. 21 23 / 29. 9 40	15·53	4·30	0·31	295	0·16	70	0·01	58
29. 22 4 / 30. 10 34	15·62	3·91	0·33	305	0·06	67	0·02	94
30. 23 20	15·69	3·73	0·23	301	0·07	116	0·01	6
July 1. 11 33								
1. 23 46	15·64	3·46	0·26	316	0·10	97	0·03	129
2. 12 23								
3. 0 59	16·11	3·38	0·25	358	0·08	158	0·08	298
3. 13 32								
4. 2 4	16·40	3·73	0·20	331	0·03	157	0·03	109
4. 14 34								
5. 3 4	15·73	4·20	0·14	312	0·03	123	0·03	83
5. 15 36 / 6. 4 6	15·34	4·65	0·19	300	0·07	101	0·03	74
6. 16 32 / 7. 4 59	15·92	5·11	0·21	331	0·07	102	0·07	69
7. 17 22 / 8. 5 40	15·76	5·70	0·28	310	0·09	92	0·01	88
8. 18 8 / 9. 6 28	15·63	6·03	0·36	299	0·10	131	0·05	31
9. 18 52 / 10. 7 16	16·01	6·25	0·25	284	0·13	38	0·02	353
10. 19 36 / 11. 8 0	16·38	6·38	0·27	308	0·06	102	0·09	103
11. 20 28 / 12. 8 51	15·72	6·21	0·32	307	0·12	98	0·07	89
12. 21 15	15·52	5·96	0·36	309	0·20	80	0·10	64
13. 9 39								
13. 22 3	15·32	5·36	0·30	309	0·17	84	0·06	64
14. 10 31								
14. 22 58	15·39	4·86	0·36	313	0·14	99	0·04	106
15. 11 28								
15. 23 58	16·05	4·31	0·34	327	0·11	108	0·02	75
16. 12 32								
17. 1 6	16·13	3·91	0·28	307	0·06	118	0·04	71
17. 13 45								
18. 2 23	16·09	3·99	0·22	331	0·05	130	0·01	38
18. 14 57								
19. 3 31	15·96	4·41	0·26	315	0·10	75	0·04	132
19. 15 59 / 20. 4 26	16·01	4·69	0·31	332	0·06	105	0·03	52
20. 16 51 / 21. 5 9	15·71	5·08	0·38	315	0·10	101	0·02	71
21. 17 31 / 22. 5 47	15·79	5·38	0·35	307	0·07	102	0·05	91
22. 18 6 / 23. 6 23	15·68	5·53	0·33	301	0·09	72	0·01	36

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 18 39	15·73	5·61	0·33	291	0·07	60	0·04	173
24. 6 55								
24. 19 7	15·75	5·58	0·30	293	0·14	48	0·07	94
25. 7 27								
25. 19 39	15·66	5·34	0·28	288	0·08	67	0·08	103
26. 7 56								
26. 20 12	15·60	5·08	0·34	287	0·13	82	0·02	238
27. 8 28								
27. 20 44	15·41	4·73	0·29	291	0·12	65	0·04	79
28. 8 59								
28. 21 18	15·54	4·48	0·26	291	0·17	51	0·04	58
29. 9 37								
29. 21 55	15·57	3·98	0·28	303	0·14	73	0·00	28
30. 10 21								
30. 22 46	15·45	3·53	0·29	302	0·08	104	0·02	325
31. 11 19								
31. 23 51	15·65	3·25	0·25	329	0·07	108	0·03	343
August 1. 12 33								
2. 1 15	15·86	3·30	0·14	357	0·07	173	0·00	262
2. 13 58								
3. 2 40	15·65	3·91	0·12	312	0·04	55	0·01	213
3. 15 8	15·52	4·59	0·23	316	0·10	150	0·03	70
4. 3 40								
4. 16 13	15·67	5·29	0·29	316	0·10	135	0·03	91
5. 4 33								
5. 17 1	15·79	6·12	0·25	298	0·06	111	0·08	86
6. 5 25								
6. 17 50	15·79	6·62	0·21	299	0·09	106	0·09	88
7. 6 10								
7. 18 34	15·78	6·89	0·30	294	0·08	110	0·06	37
8. 6 57								
8. 19 19	15·95	6·93	0·30	275	0·04	140	0·09	34
9. 7 40								
9. 20 2	15·89	6·63	0·29	289	0·11	75	0·04	38
10. 8 26								
10. 20 46	15·64	6·20	0·36	297	0·15	79	0·06	46
11. 9 7								
11. 21 28	15·68	5·60	0·28	298	0·17	77	0·11	100
12. 9 54								
12. 22 20	15·43	4·72	0·35	312	0·15	83	0·04	131
13. 10 51								
13. 23 21	15·31	3·90	0·37	314	0·11	91	0·02	25
14. 12 1								
15. 0 40	15·46	3·50	0·26	343	0·10	134	0·07	30
15. 13 23								
16. 2 6	15·58	3·60	0·19	329	0·05	158	0·05	24
16. 14 40								
17. 3 13	15·84	4·14	0·22	317	0·05	90	0·03	33
17. 15 44	15·99	4·47	0·23	339	0·08	50	0·06	152
18. 4 14								
18. 16 27	16·01	5·02	0·25	312	0·11	73	0·04	99
19. 4 48								
19. 17 10	15·80	5·30	0·34	311	0·08	96	0·09	69
20. 5 24								
20. 17 40	15·65	5·60	0·27	296	0·05	44	0·08	73
21. 5 56								
21. 18 14	15·31	5·80	0·33	291	0·07	64	0·08	51
22. 6 26								
22. 18 40	15·83	5·78	0·28	295	0·10	63	0·09	54
23. 6 58								

Height of the Water in each individual tide at Castle Townsend, excluding diurnal tide,

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
• June 22. 17 1	15·86	4·31	0·15	283	0·07	208	0·02	170
23. 5 20								
23. 17 45	15·77	4·49	0·17	276	0·02	77	0·05	277
24. 6 10								
24. 18 36	15·72	4·21	0·27	266	0·07	259	0·03	13
25. 6 46								
25. 19 7	15·41	4·15	0·21	271	0·03	239	0·03	203
26. 7 21								
26. 19 41	15·21	3·98	0·23	292	0·02	18	0·02	3
27. 8 0								
27. 20 20	15·20	3·65	0·17	277	0·04	243	0·01	309
28. 8 40								
28. 21 0	15·30	3·37	0·16	315	0·06	51	0·13	63
29. 9 17								
29. 21 40	15·43	3·11	0·16	306	0·04	269	0·02	182
30. 10 10								
30. 22 39	15·29	2·83	0·19	317	0·05	315	0·02	147
July 1. 11 8								
1. 23 37	15·44	2·70	0·16	323	0·01	221	0·02	266
2. 12 6								
3. 0 35	15·92	2·80	0·04	259	0·09	263	0·04	338
3. 13 12								
4. 1 48	16·04	3·01	0·15	345	0·14	295	0·02	35
4. •14 15								
5. 2 42	15·42	3·37	0·17	299	0·07	328	0·04	190
5. 15 12								
6. 3 41	15·16	3·85	0·12	312	0·08	259	0·09	31
6. 16 2								
7. 4 24	15·42	4·34	0·17	276	0·06	344	0·01	61
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
8. 17 35	15·27	5·06	0·19	296	0·06	265	0·06	294
9. 5 55								
9. 18 10	15·34	5·06	0·37	325	0·17	203	0·07	34
10. 6 34								
10. 18 59	15·92	5·28	0·18	280	0·04	313	0·06	309
11. 7 23								
11. 19 50	15·35	5·11	0·27	296	0·04	274	0·04	106
12. 8 13								
12. 20 42	15·12	4·77	0·24	269	0·05	264	0·09	256
13. 9 6								
13. 21 32	14·85	4·32	0·26	287	0·06	222	0·10	30
14. 9 59								
.....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....
15. 23 29	15·77	3·47	0·17	262	0·11	285	0·07	16
16. 12 6								
17. 0 43	15·65	3·55	0·18	1	0·13	77	0·13	108
17. 13 22								
18. 2 0	15·72	3·31	0·18	295	0·06	284	0·04	154
18. 14 33								
19. 3 5	15·66	3·61	0·19	307	0·03	323	0·05	202
19. 15 32								
20. 3 59	15·66	3·92	0·27	316	0·07	329	0·01	337
20. 16 26	15·47	4·16	0·25	318	0·04	52	0·04	137
21. 4 44								
21. 17 2	15·59	4·31	0·17	303	0·08	237	0·03	243
22. 5 18								
22. 17 35	15·41	4·40	0·23	281	0·05	116	0·01	184
23. 5 52								

expressed by the formula $A_0+C_1\sin p+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$.

Analysed time of high water, corresponding to $p=90°$.	A_0.	C_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	ft.	°	ft.	°	ft.	°
July 23. 18 5	15·45	4·43	0·15	279	0·13	125	0·06	67
24. 6 21								
24. 18 36	15·43	4·43	0·31	281	0·06	179	0·05	351
25. 6 56								
25. 19 14	15·39	4·27	0·18	293	0·08	307	0·03	281
26. 7 31								
26. 19 46	15·38	4·04	0·23	293	0·04	233	0·08	7
27. 8 2								
27. 20 19	15·20	3·71	0·23	282	0·11	271	0·03	78
28. 8 34								
28. 20 57	15·36	3·42	0·33	295	0·07	227	0·14	122
29. 9 16								
29. 21 40	15·31	3·02	0·19	274	0·05	281	0·04	79
30. 10 5								
30. 22 39	15·29	2·71	0·20	318	0·01	90	0·07	118
31. 11 13								
31. 23 46	15·41	2·58	0·15	322	0·01	224	0·01	247
August 1. 12 27								
2. 1 7	15·61	2·70	0·07	354	0·06	311	0·02	359
2. 13 44								
3. 2 20	15·43	3·20	0·08	332	0·01	110	0·02	221
3. 14 50								
4. 3 20	15·34	3·87	0·03	295	0·03	146	0·06	52
4. 15 45								
5. 4 9	15·40	4·49	0·15	333	0·10	285	0·04	108
5. 16 41	15·52	5·07	0·16	263	0·09	57	0·09	300
6. 5 5								
6. 17 33	15·51	5·44	0·25	32	0·06	328	0·03	100
7. 5 53								
7. 17 56	15·47	5·70	0·24	299	0·19	231	0·12	349
8. 6 19								
8. 18 37	15·65	5·80	0·30	282	0·07	323	0·04	300
9. 6 58								
9. 19 26	15·59	5·41	0·29	277	0·07	163	0·01	315
10. 7 50								
10. 20 13	15·49	4·90	0·25	318	0·14	284	0·07	295
11. 8 39								
11. 21 9	15·33	4·34	0·20	301	0·09	326	0·03	348
12. 9 32								
12. 21 58	15·22	3·60	0·21	294	0·06	283	0·02	216
13. 10 31								
13. 23 7	15·19	3·18	0·26	323	0·03	24	0·02	96
14. 11 50								
15. 0 33	15·23	2·85	0·20	312	0·08	333	0·07	41
15. 13 13								
16. 1 53	15·33	2·90	0·12	317	0·03	230	0·05	18
16. 14 28								
17. 3 2	15·58	3·30	0·16	307	0·04	321	0·00	329
17. 15 27								
18. 3 52	15·74	3·72	0·17	311	0·03	3	0·10	74
18. 16 10								
19. 4 28	15·61	4·04	0·18	298	0·09	259	0·04	55
19. 16 49	15·58	4·28	0·27	306	0·05	276	0·07	268
20. 5 3								
20. 17 21	15·43	4·42	0·29	279	0·06	217	0·06	146
21. 5 37								
21. 17 50	15·19	4·67	0·23	299	0·04	28	0·05	149
22. 6 2								
22. 18 13	15·48	4·61	0·27	283	0·04	20	0·05	330
23. 6 31								

Section XI.—*Discussion of the height of mean water deduced from the analysis of individual tides; with reference to difference of station, and to variations of the phase of the moon, and of the declination of the moon.*

The mean heights (A_0) for each station, in the results of last section, were divided into groups corresponding to large tides and small tides, the dividing places being the same as those in section V., page 29, or following by two days the times when the moon's hour-angle from the sun was 3^h, 9^h, 15^h, 21^h. The means for each group were taken, and then the mean of those means of groups belonging to large tides, and the similar mean for small tides. The results are the following, the numbers for Courtown being taken by anticipation from section XVII.:—

Station.	Mean height.		Mean of mean heights.	Excess of mean height in large tides above mean height in small tides.
	Small tides.	Large tides.		
	ft.	ft.	ft.	ft.
Kilbaha	15·68	15·72	15·65	0·14
Kilrush	16·36	16·52	16·44	0·16
Foynes Island....	17·06	17·29	17·18	0·23
Limerick.....	17·38	17·92	17·65	0·54
Casleh Bay.....	17·90	18·06	17·98	0·16
Galway	17·47	17·65	17·56	0·18
Old Head	17·69	17·90	17·80	0·21
Mullaghmore	18·03	18·23	18·13	0·20
Buncrana	17·33	17·47	17·40	0·14
Port Rush	17·60	17·83	17·72	0·23
Carrowkeel.....	17·96	18·17	18·07	0·21
Ballycastle	17·40	17·63	17·52	0·23
Glenarm	17·65	17·83	17·74	0·18
Donaghadee	17·76	17·89	17·83	0·13
Ardglass.....	17·65	17·78	17·72	0·13
Clogher Head....	17·20	17·32	17·26	0·12
Kingstown	17·44	17·49	17·47	0·05
Courtown	16·63	16·67	16·65	0·04
Dunmore East ..	15·80	15·86	15·83	0·06
New Ross	16·58	16·89	16·74	0·31
Passage West....	15·71	15·78	15·75	0·07
Castle Townsend.	15·45	15·45	15·45	0·00

The first column which deserves attention is the "mean of mean heights." The progress of the numbers from Kilbaha to Limerick, as well as that from Dunmore East to New Ross, show well the change of mean height in ascending a river affected by current as well as by tide. Excluding these river stations, as also Buncrana and Carrowkeel, which partake in some measure of the same character, we have a view of the comparative mean heights of the sea on different parts of the coast of Ireland. And here we have the remarkable result, to which allusion has already been made, that the mean height of the sea round the northern half of the island, as referred to the surface of stagnant water, is considerably greater than that round the southern half of the island. The amount of this difference of height is believed by the officers who directed the levelling operations to be much greater than can be explained by any allowable error in the levelling. The heights on the eastern coast are also, perhaps, a little greater than those on the western coast. I profess myself entirely unable to explain on mechanical principles this result.

In every instance except that of Castle Townsend, the mean height in large tides is greater than that in small tides. Further allusion will be made to this in the examination of the next table. I shall here only remark that I imagine this to be a possible result of the shallowness of the sea, though theory has not yet reached so far.

For the investigation of the effect of the moon's declination, the same process in all respects was used as in Section V., pages 32 and 33, and the following are the results:—

Station.	Mean height with small declination.	Mean height with large declination.	Excess of mean height with large declination above mean height with small declination.	Mean height with decreasing declination.	Mean height with increasing declination.	Excess of mean height with decreasing declination above mean height with increasing declination.
	ft.	ft.	ft.	ft.	ft.	ft.
Kilbaha	15·61	15·66	+0·05	15·68	15·59	+0·09
Kilrush	16·40	16·46	+0·06	16·52	16·34	+0·18
Foynes Island....	17·15	17·18	+0·03	17·24	17·07	+0·17
Limerick.....	17·68	17·57	−0·11	17·81	17·43	+0·38
Casleh Bay.....	17·94	18·00	+0·06	18·03	17·91	+0·12
Galway	17·54	17·58	+0·04	17·62	17·49	+0·13
Old Head	17·71	17·81	+0·10	17·91	17·69	+0·22
Mullaghmore....	18·07	18·15	+0·08	18·22	18·06	+0·16
Buncrana	17·37	17·42	+0·05	17·53	17·31	+0·22
Port Rush	17·68	17·70	+0·02	17·88	17·59	+0·29
Carrowkeel.....	17·97	18·12	+0·15	18·18	17·94	+0·24
Ballycastle.....	17·47	17·59	+0·12	17·64	17·39	+0·25
Glenarm.....	17·68	17·76	+0·08	17·87	17·66	+0·21
Donaghadee	17·79	17·85	+0·06	17·93	17·77	+0·16
Ardglass.....	17·69	17·73	+0·04	17·82	17·64	+0·18
Clogher Head ..	17·21	17·29	+0·08	17·29	17·20	+0·09
Kingstown	17·41	17·50	+0·09	17·53	17·42	+0·11
Courtown	16·60	16·67	+0·07	16·70	16·63	+0·07
Dunmore East ..	15·80	15·88	+0·08	15·90	15·79	+0·11
New Ross	16·74	16·68	−0·06	16·85	16·61	+0·24
Passage West....	15·70	15·77	+0·07	15·77	15·70	+0·07
Castle Townsend..	15·42	15·47	+0·05	15·46	15·44	+0·02

Upon comparing the results of this Table with those of the Table on page 96, the remarks made in page 33 must, I think, be considered to be insufficiently founded. The excess found by classifying according to the magnitude of the tide is here decidedly greater than that found by classifying according to the moon's declination. In the river tides (from Kilbaha to Limerick, and from Dunmore East to New Ross) the excess classified by the magnitude of tide proceeds more regularly than that classified by moon's declination. I think also that the change for the stations on the narrow channel from Port Rush to Donaghadee inclines us to the supposition that the whole is due rather to the variation of magnitude than to the variation of declination. The change of declination, being very slow, would probably produce the same sensible change in the mean level of the deep (though contracted) *sea* of the North Channel as in that of the Atlantic Ocean: though everything which depended in any way upon the tides might be very different. Perhaps however the change for these stations is not sufficiently decided to give great force to this argument. On the whole, I regard the origin of this inequality as yet subject to some doubt.

The values of A_0 at any one station differ, sometimes rapidly, from day to day. In order to examine these, I have subtracted from every value of A_0 at each place the

mean of all the values of A_0 at that place, and have set down the excess as an irregularity in the general height of the water at that place on that day. The numbers for Courtown are taken by anticipation from Section XVII. The results are contained in the following Table. With the view of ascertaining any possible connexion of these irregularities with the cause of the winds, I have set down in the last columns the character of the winds observed at the four stations Kilbaha, Port Rush, Kingstown, and Passage West, which may be considered as nearly equidistant on the coast. [The letter *c* attached to the letters describing the direction of the wind denotes that it was nearly calm; the letter *s* denotes that the wind was strong.]

Approximate time of high water at Kilbaha.		Excess of each value of A_0 above the mean value of A_0 at every station.																					Wind.			
		Kilbaha.	Kilrush.	Foynes Island.	Limerick.	Casleh Bay.	Galway.	Old Head.	Mullaghmore.	Buncrana.	Port Rush.	Carrowkeel.	Ballycastle.	Glenarm.	Donaghadee.	Ardglass.	Clogher Head.	Kingstown.	Courtown.	Dunmore East.	New Ross.	Passage West.	Castle Townsend.	Kilbaha.	Port R	Kingstown.	Passage West.
	hrs.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.				
June 22.	18							+0·71	+0·31	−0·17	+0·19	+0·03			+0·39	+0·63		+0·52	+0·30	+0·46	+0·58		+0·41		S.S.W.		
	23.	6																							S.W.	W.	
	23.	18		+0·70			+0·59		+0·75	+0·72	+0·98	+0·90	+0·62		+0·90	+0·87	+0·88	+0·23	+0·67	+0·67					S.W.*s.*	S.	
	24.	7																			+0·55	+0·73		+0·32		S.W.*s.*		W.
	24.	18	+0·75	+0·76			+0·77	+1·03	+0·69	+0·99	+0·82	+0·92	+0·51		+0·89	+0·95		+0·67	+0·65	+0·95	+0·40				S.W.*s.*	S.W.*s.*		S.W.
	25.	9																			+0·54	+0·73	+0·38	+0·27		N.W.*s.*	W.	W.
	25.	19	+0·14	+0·18	+0·39	+0·76	+0·16	+0·28	+0·08	+0·16	+0·53	+0·54	+0·19		+0·41	+0·51	+0·48	+0·22	+0·25	+0·40	+0·42				N.W.	N.W.	W.*s.*	W.
	26.	8																			+0·19	+0·06		−0·04	N.W.	N.W.*s.*	W.*s.*	W.N.W.
	26.	20	−0·15	−0·15	−0·13	−0·11	−0·13	−0·12	−0·27	−0·12	+0·04	−0·06	−0·25		−0·26	−0·27	−0·10	−0·11	−0·12	−0·08	−0·12	−0·17			N.W.	N.W.	W.*s.*	W.
	27.	8																			−0·08		−0·23	−0·24	S.W.	N.W.	S.*c.*	W.
	27.	21	0·00	−0·04	+0·03	+0·06	+0·10	+0·18		+0·07	+0·26	+0·20	−0·17	−0·07	+0·13	+0·18	+0·11	+0·09	−0·23		−0·14	−0·13			S.W.	W.	N.W.*c.*	S.W.
	28.	9																			−0·10		−0·20	−0·25	S.W.	N.W.	W.*c.*	S.W.
	28.	21	−0·13	+0·04	−0·10	−0·15	−0·18	−0·17	−0·07	+0·12	+0·07	−0·20	−0·15	−0·17	−0·16	−0·02	+0·04	+0·03	−0·05	+0·03	+0·01	−0·17			N.W.	N.W.	W.	S.W.
	29.	9																			−0·09		−0·22	−0·15	N.W.	N.W.	W.*c.*	N.W.
	29.	22	0·00	−0·08	−0·15	−0·41	−0·05		−0·15	−0·05	0·00	−0·31	−0·09	−0·28	−0·42	−0·14	−0·13	−0·13	−0·16	+0·01	+0·05				N.E.	N.E.	S.E.*c.*	W.
	30.	10																		+0·08	+0·07	−0·21	−0·13	−0·02	N.E.*c.*	N.E.	S.W.	S.W.
	30.	22	+0·05	+0·08	+0·07	+0·47	+0·12	+0·14	+0·11	+0·20	+0·31	+0·23	+0·27	+0·05	−0·19	+0·02	−0·01	0·00	+0·01	−0·04					S.W.*c.*	S.E.	S.*c.*	S.W.
July	1.	11																		+0·06	+0·01	−0·19	−0·06	0·16	N.W.	S.W.	W.	W.
	1.	23	−0·14	−0·14	−0·04	+0·33	−0·24	+0·02	−0·11	+0·33	+0·12	+0·14		−0·23	−0·15	+0·10	+0·05	−0·05	+0·03	+0·02	+0·02				W.*c.*	N.W.	N.W.*s.*	W.N.W.
	2.	12																		+0·03	−0·02	−0·24	−0·11	−0·01	S.W.*c.*	N.W.	W.	W.
	3.	1	+0·31	+0·30	+0·25	+0·13	+0·28	+0·38	+0·22	+0·22	+0·30	+0·21	+0·28	−0·22	−0·03	+0·17	+0·09	+0·08	+0·13		−0·07				S.E.*s.*	W.S.W.*s.*	S.E.	S.E.
	3.	13																		+0·25	+0·45	+0·22	+0·36	−0·47	S.W.*c.*	S.W.	S.*c.*	
	4.	2	+0·76	+0·81	+0·86	+0·80	+0·85	+0·95	+0·88	+0·84	+0·78	+0·74	+0·51	+0·42	+0·73	+0·87	+0·77	+0·62	+0·71						S.W.	S.W.	S.W.*s.*	S.W.
	4.	14																		+0·96	+0·80	+0·82	+0·65	+0·59		S.W.	S.*c.*	W.S.W.
	5.	3	−0·16	+0·07	+0·19		−0·10	0·00	+0·06	+0·13	+0·39	+0·38	+0·47	+0·58	+0·57	+0·62	+0·50	+0·26	+0·39						N.W.	N.W.*s.*	W.	N.W.
	5.	15																		+0·77	+0·21	+0·45	−0·02	−0·03	N.W.*c.*	N.W.	W.*s.*	N.W.
	6.	4	−0·22	−0·24	−0·30	−0·37	−0·18		−0·37	−0·17	−0·34	−0·37	−0·32	−0·36	−0·40	−0·25	−0·31	−0·25	−0·29	−0·31					S.W.*c.*	N.E.	W.*c.*	S.W.
	6.	16	+0·35	+0·31	+0·30	+0·46	+0·37	+0·42		+0·74	+0·15	+0·16	+0·34	+0·25	+0·04	+0·18		+0·14	+0·08		−0·25	−0·44	−0·41	−0·29	S.	S.W.	E.	S.W.
	7.	4																		+0·58					W.*s.*		S.W.*s.*	N.W.
	7.	17	+0·40	+0·46	+0·49	+1·00	+0·41	+0·53	+0·25	+0·29	+0·30	+0·28	+0·54	+0·30	+0·25	+0·38	+0·51	+0·35	+0·40			+0·20	+0·17	−0·03	N.W.*s.*	S.E.	S.W.	S.W.
	8.	5																			+0·23	+0·37	+0·01		N.W.*s.*	E.N.E.	S.E.*s.*	N.W.
	8.	18	+0·31	+0·19	+0·22	+0·71	+0·24	+0·35	+0·43	+0·60	+0·38	+0·43	+0·45	+0·36	+0·19	+0·17	+0 04	+0·27	+0·06						N.W.	S.E.	S.	S.W.
	9.	6																		+0·02	+0·10	+0·12	−0·12	−0·18	N.W.	S.E.	W.	W.
	9.	18	+0·33	+0·42	+0·20	+0·53	+0·23	+0·31	+0·42	+0·52	+0·45	+0·56	+0·29	+0·48	+0·37	+0·35	+0·18	+0·28	+0·22	−0·01					S.W.	W.	W.	W.S.W.
	10.	7																			+0·17	+0·29	+0·26	−0·11	S.W.	W.	S.W.*s.*	S.S.W.
	10.	19	+0·47	+0·58	+0·52	+0·75	+0·39	+0·35	+0·35	+0·44	+0·58	+0·57	+0·41	+0·60	+0·56	+0·54	+0·57	+0·69	+0·57	+0·99					N.W.	N.E.	S.W.*c.*	N.W.
	11.	8																			+0·58	+1·00	+0·63	+0·47	N.W.	N.E.	N.W.	N.W.
	11.	20	+0·14	+0·26	+0·25	+0·35	+0·18	+0·16	+0·13	−0·12	−0·08	+0·11	−0·16	+0·07	+0·13	+0·10	+0·03	+0·03	−0·10	−0·15					N.W.	E.S.E.	S.W.*c.*	W.
	12.	8																			−0·10	+0·30	−0·03	−0·10	S.W.	S.W.	S.	S.W.
	12.	21	−0·19	−0·09	+0·22	+0·92	−0·04	+0·04	+0·03	−0·09	−0·09	+0·43	−0·05	+0·14	+0·39	+0·33	+0·15	+0·08	−0·07	−0·04	−0·19	+0·23	−0·23	−0·33	S.W.	S.W.	S.W.*s.*	S.W.
	13.	9																							S.W.	S.W.	S.W.	S.W.
	13.	22	−0·39	−0·44	−0·41	−0·20	−0·41		−0·49	−0·52	−0·41	−0·35	−0·61	−0·38	−0·12	−0·24	−0·18	−0·54	−0·28		−0·47	−0·35	−0·43	−0·60	N.W.	W.S.W.	W.	W.
	14.	10																							N.W.	W.S.W.	S.W.	N.W.
	14.	23	−0·26	−0·34	−0·47	−0·74	−0·40	−0·43	−0·28	−0·40	−0·59	−0·68	−0·66	−0·65	−0·61	−0·55	−0·48	−0·42	−0·40	−0·55	−0·32	−0·51	−0·36		S.E.	S.E.	S.	S.E.
	15.	11																							S.E.*s.*	S.E.	S.	S.E.
	15.	23	+0·36	+0·19	−0·02	−0·51	+0·27	+0·05	+0·30	+0·02	−0·09	−0·20	−0·08	−0·07	−0·27	−0·14	−0·10	−0·12	+0·03	−0·05	+0·19	+0·07	+0·30	+0·32	S.E.*s.*	S.E.	E.*s.*	S.E.
	16.	12																							S.E.	S.E.*s.*	S.E.*s.*	S.
	17.	1	+0·35	+0·26	+0·17	−0·10	+0·29	+0·15	+0·28	+0·14	+0·13	+0·01	+0·39	+0·29	+0·07	+0·13		−0·16	+0·23	+0·42	+0·37	+0·24	+0·38	+0·20	S.W.	S.E.*s.*	S.*s.*	S.W.
	17.	13																		+0·48					S.W.*c.*	S.E.	S.W.*s.*	S.W.
	18.	2	+0·15	+0·16	+0·11	−0·27	+0·22	+0·05	+0·15	+0·09	−0·12	−0·21	+0·33	+0·04	−0·10	+0·10	+0·05	+0·04	+0·23	+0·26	+0·28	+0·18	+0·34	+0·27	S.W.*c.*	E.S.E.	S.E.*s.*	S.W.
	18.	14																		+0·27					N.E.	S.E.	S.W.*c.*	S.W.
	19.	3	+0·07	+0·01	0·00	−0·27	+0·10		+0·08	−0·08	−0·11	−0·31		−0·04	−0·14	−0·09	+0·02	−0·12	+0·06	+0·18	+0·22	+0·05	+0·21	+0·21	N.E.*c.*	E.N.E.*s.*	S.E.*s.*	S.E.
	19.	16	+0·11	+0·02	0·00	−0·20	0·00	−0·04	+0·03	−0 04	−0·12	−0·16	+0·07	−0·02	−0·29	−0·17	−0·08	0·00	+0·11	+0·19					N.E.*c.*	N.E.	S.S.W.*c.*	S.W.
	20.	4																		+0·07	+0·22	+0·05	+0·26	+0·11	N.E.*c.*	N.E.	N.E.*s.*	N.
	20.	16	−0·11	−0·20	−0·18	−0·19	−0·10	−0·25	−0·19	−0·10	−0·24	−0·15	−0·06	−0·13	−0·44	−0·19	−0·12	−0·11	−0·13	+0·17					N.E.*c.*	N.W.	N.*s.*	N.W.
	21.	5																		−0·02	−0·05	−0·10	−0·04	+0·02	N.N.W.*c.*	N.W.	S.E.*c.*	N.W.
	21.	17	−0·09	−0·15	−0·16	−0·15	−0·15	−0·20	−0·24	−0·45	−0·36	−0·25	−0·15	−0·22	−0·46	−0·25	−0·18	−0·16	−0·13	−0·16	0·00	−0·05	+0·04	+0·14	S.E.*c.*	N.W.	S.W.	N.W.
	22.	6																							N.E.*c.*	N.W.	S.E.	S.W.
	22.	18	−0·14	−0·16	−0·10	−0·07	−0·11	−0·13	−0·19	−0·26	−0·24	−0·15	−0·09	−0·18	−0·48	−0·33	−0·26	−0·15	−0·25	−0·31	−0·10	−0·10	−0·07	−0·04	N.E.*c.*	S.W.	S.S.E.	N.W.
	23.	6																							N.W.*c.*	E.N.E.	S.E.*c.*	S.S.E.

Approximate time of high water at Kilbaha.		Excess of each value of A_0 above the mean value of A_0 at every station.																					Wind.			
		Kilbaha.	Kilrush.	Foynes Island.	Limerick.	Casleh Bay.	Galway.	Old Head.	Mullaghmore.	Buncrana.	Port Rush.	Carrowkeel.	Ballycastle.	Glenarm.	Donaghadee.	Ardglass.	Clogher Head.	Kingstown.	Courtown.	Dunmore East.	New Ross.	Passage West.	Castle Townsend.	Kilbaha.	Port Rush.	Kingstown.	Passage West.
	hrs.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.				
July 23.	18	−0·18	−0·13	−0·19	−0·10	−0·05	−0·23	−0·23	−0·22	−0·19	−0·15	−0·15	−0·23	−0·46	−0·29	−0·24	−0·29	−0·20	−0·31	−0·11	−0·15	−0·02	0·00	N.E.*C.*	N.E.	E.*C.*	N.W.
24.	7																							N.E.*C.*	N.E.	S.W.	S.W.
24.	19	−0·07	−0·08	−0·12	−0·23	−0·12	−0·24	−0·08	−0·14	−0·14	−0·20	−0·11	−0·27	−0·40	−0·28	−0·24	−0·02	−0·30	−0·12	−0·01	−0·15	0·00	−0·02	N.W.*C.*	N.W.	N.W.	N.W.
25.	7																							N.W.*C.*	N.W.	E.*C.*	S.W.
25.	19	−0·17	−0·23	−0·25	−0·31	−0·27	−0·34	−0·29	−0·51	−0·44	−0·35	−0·28	−0·41	−0·39	−0·26	−0·20	−0·27	−0·20	−0·26	−0·06	−0·15	−0·09	−0·06	E.*C.*	N.W.	N.	N.W.
26.	8																		−0·26					E.*C.*	N.W.	N.W.*C.*	N.N.W.
26.	20	−0·34	−0·46	−0·41	−0·55	−0·42	−0·60	−0·38	−0·50	−0·53	−0·50	−0·52	−0·54	−0·58	−0·40	−0·32	−0·32	−0·24	−0·22	−0·18	−0·26	−0·15	−0·07	E.	N.W.	N.W.	N.W.
27.	8																		−0·29					N.E.	N.W.	N.E.	N.W.
27.	21	−0·37	−0·49	−0·40	−0·46	−0·41	−0·58	−0·38	−0·36	−0·36	−0 34	−0·47	−0·29	−0·51	−0·25	−0·25	−0·15	−0·23	−0·21	−0·26	−0·38	−0·34	−0·25	N.*C.*	W.N.W.*S.*	W.*S.*	N.E.
28.	9																		−0·26					N.*C.*	W.N.W.*S.*	N.W.*S.*	N.W.
28.	21	−0·48	−0·64	−0·58	−0·79	−0·54		−0·54	−0·56	−0·58	−0·66	−0·51	−0·46	−0·77	−0·55	−0·43	−0·24	−0·31	−0·19	−0·29	−0·54	−0·21	−0·09	N.E.	N.E.*S.*	N.*S.*	N.N.E.
29.	9																		−0·27					N.E.*C.*	N.E.*S.*	N.*S.*	N.N.E.
29.	22	−0·33	−0·42	−0·40	−0·44	−0·36	−0·47	−0·45	−0·49	−0·38	−0·54	−0·57	−0·35	−0·67	−0·44	−0·37	−0·32	−0·30	−0·25	−0·17	−0·50	−0·18	−0·14	N.W.*C.*	N.W.	N.W.*S.*	N.N.E.
30.	10																		−0·32					N.W.*C.*	N.W.	N.W.*S.*	N.W.
30.	22	−0·40	−0·46	−0·50	−0·71	−0·48	−0·52	−0·51	−0·63	−0·34	−0·32	−0·38	−0·41	−0·40	−0·44	−0·38	−0·45	−0·35	−0·38	−0·32	−0·58	−0·30	−0·16	N.*C.*	N.W.	N.W.*S.*	N.
31.	11																		−0·42					N.W.*C.*	N.W.	S.S.E.*C.*	N.W.
31.	23	−0·20	−0·30	−0·29	−0·54	−0·27	−0·31	−0·24	−0·42	−0·16	−0·29	−0·10	−0·45	−0·26	−0·28	−0·34	−0·27	−0·28	−0·26	−0·19	−0·39	−0·10	−0·04	S.E.*C.*	S.W.	S.E.*C.*	S.W.
August 1.	12																		−0·10					S.W.	W.S.W.*S.*	N.E.*C.*	S.W.
2.	1	+0·10	+0·15	+0·02	−0·21	+0·12	+0·10	+0·05	+0·11	+0·28	+0·12	−0·09	−0·03	+0·30	+0.14	−0·01	+0·03	+0·01	−0·06	+0·06	−0·08	+0·11	+0·16	S.W.*C.*	W.S.W.*S.*	S.*S.*	S.S.W.
2.	14																		−0·21					N.W.	N.W.	S.W.*C.*	S.E.
3.	3	−0·27	−0·12	−0·18	−0·53	−0·25	−0·19	−0·25	−0·20	−0·16	−0·38	−0·28	−0·21	+0·12	−0·05	−0·14	+0·12	−0·14	−0·31	−0·11	−0·34	−0·10	−0·02	N.	N.E.	N.E.	N.W.
3.	15																		−0·15					S.W.	S.W.*S.*	W.	S.W.
4.	4	−0·20	−0·10	−0·02	0·00	−0·06	+0·06	−0·16		+0·20	+0·05	+0·15	+0·26	+0·35	+0·15	+0·13	+0·23	+0 05	−0·16	−0·14	−0·24	−0·23	−0·11	N.W.	W.N.W.*C.*	W.*S.*	N.W.
4.	16																		+0·13					S.W.	S.W.*S.*	W.*S.*	S.W.
5.	5	+0·03	+0·20	+0·20	+0·44	+0·19		+0·52	+0·27	+0·39	+0·44	+0·32	+0·54	+0·60	+0·41	+0·31	−0·31	+0·28	+0·10	+0·04	+0·08	−0·08	−0·05	N.W.	W.S.W.*S.*	W.*S.*	N.W.
5.	17																		−0·05					W.S.W.	W.N.W.*S.*	S.W.	W.
6.	5	+0 05	+0·24	+0·20	+0·37	+0·17	+0·49	+0·40	+0·51	+0·37	+0·28	+0·36	+0·35	+0·36	+0·16	+0·19	+0·18	+0·12		+0·08	+0·04	+0·04	+0·07	S.W.	W.N.W.*S.*	S.W.*S.*	S.W.
6.	18																							S.W.*S.*	S.W.*S.*	S.S.E.*C.*	S.W.
7.	6	+0·15	+0·36	+0·35	+0·76	+0·31	+0·46		+0·60	+0·42	+0·47	+0·52	+0·59	+0·58	+0·35	+0·33	+0·27	+0·23	+0·02	+0·10	+0·31	+0·04	+0·06	S.W.	W.S.W.*S.*	S.W.	W.
7.	19																							S.	S.E.*S.*	E.	S.W.
8.	7	+0·24	+0·32	+0·59	+0·34	+0·34	+0·37		0·00	+0·27	+0·34	+0·53	+0·31	+0·43	+0·05	+0·11	−0·10	−0·09	−0·17	+0·03	+0·41	+0·03	+0·02	S.W.	S.W.	S.S.E.*S.*	S.W.
8.	19																							S.*C.*	S.W.	E.	S.W.
9.	8	+0·29	+0·25	+0·38	+0·42	+0·20	+0·21	+0·18	+0·17	+0·31	+0·31	+0·35	+0·36	+0·45	+0·19	+0·16	−0·08	+0·15	+0·01	+0·16	+0·39	+0·20	+0·20	S.W.	E.S.E.*S.*	S.*S.*	S.W.
9.	20																							N.*C.*	S.W.*C.*	S.E.	S.W.
10.	8	+0·11	+0·07	+0·30	+0·51	+0·10	+0·06	+0·12	+0·41	+0·31	+0·19	+0·16	+0·06	+0·52	+0·20	+0·13	+0·11	+0·14	+0·68	+0·17	+0·40	+0·14	+0·14	N.W.*C.*	N.W.	W.*S.*	N.N.W.*S.*
10.	21																							S.W.*C.*	W.S.W.	N.W.*S.*	W.S.W.
11.	9	−0·02	−0·08	−0·05	+0·08	−0·02	0·00	−0·14	−0·16	−0·03	−0·15	−0·28	−0·18	+0·33	+0·10	+0·09	+0·28	+0·03	−0·07	−0·15	+0·03	−0·11	+0·04	N.W.*C.*	N.W.	S.E.	S.W.
11.	21																							S.W.*C.*	S.W.*S.*	S.	S.W.
12.	10	−0·01	−0·06	+0·04	+0·12	+0·01		−0·10			+0·11	−0·31	−0·01	+0·32	+0·10	+0·16	0·00	+0·09	−0·01	−0·11	+0·14	−0·07	−0·12	S.W.*C.*	W.S.W.	S.E.	S.W.
12.	22																		0·00					S.W.*C.*	W.S.W.*S.*	S.W.	S.W.
13.	11	−0·24	−0·27	−0·25	−0·47	−0·15	−0·35	−0·23	−0·11	−0·08	−0·14	−0·45	+0·07	+0·15	−0·07	−0·02	+0·01	−0·11	−0·37	−0·30	−0·41	−0·32	−0·23	S.W.*C.*	W.S.W.*S.*	S.E.	S.W.
13.	23																		−0·34					S.W.*C.*	W.S.W.*S.*	S.E.	S.W.
14.	12	−0·38	−0·46	−0·46	−0·78	−0·40	−0·36	−0·36	−0·34	−0·31	−0·33	−0·55	+0·02	−0·20	−0·33	−0·30	+0·10	−0·30	−0·43	−0·44	−0·64	−0·44	−0·26	S.W.*C.*	S.W.*S.*	S.E.*C.*	S.W.
15.	0																		−0·34					S.W.*C.*	S.W.*C.*	W.*C.*	S.W.
15.	13	−0·38	−0·37	−0·46	−0·79	−0·34	−0·44	−0·47	−0·49	−0·44	−0·49	−0·47	−0·24	−0·29	−0·37	−0·36	−0·10	−0·25	−0·31	−0·27	−0·48	−0·29	−0·22	N.W.*C.*	N.W.	S.S.E.	N.W.
16.	1																		−0·28					N.E.*C.*	N.W.	N.E.*C.*	N.W.
16.	15	−0·24	−0·20	−0·41	−0·80	−0·25	−0·33	−0·28	−0·43	−0·41	−0·51	−0·30	−0·36	−0·39	−0·48	−0·42	−0·15	−0·34	−0·08	−0·26	−0·46	−0·17	−0·12	*E.*	N.E.	N.E.*C.*	S.E.
17.	3																		−0·32					S.E.	S.S.E.	N.E.	S.E.
17.	15	−0·06	+0·09	−0·12	−0·45	−0·03		−0·01	−0·14	+0·06	−0·04	+0·02	0·00	0·00	−0·02	−0·01	+0·22	+0·16	+0·15	+0·17	−0·22	+0·09	+0·13		N.E.	E.	S.
18.	4																		−0·05					N.E.*C.*	N.E.	E.*C.*	S.E.
18.	16	−0·01	−0·05	−0·07	−0·32	+0·01	−0·13	−0·11	−0·14	−0·03	−0·10	0·00	−0·02	−0·03	−0·09	−0·06	−0·03	+0·02		+0·20	−0·11	+0·24	+0·29		N.E.	S.E.*C.*	S.
19.	5																							N.W.*C.*	N.E.	S.E.*C.*	N.W.
19.	17	+0·11	+0·11	+0·10	+0·08	+0·14	+0·23	+0·10	+0·13	+0·14	+0·16	+0·17	+0·25	+0·21	+0·15	+0·19	+0·24	+0·20	+0·44	+0·32	+0·13	+0·26	+0·16	N.W.*C.*	N.E.	S.	N.W.
20.	5																							S.	S.E.*S.*	S.E.*C.*	S.W.
20.	18	+0·09	+0·09	+0·07	+0·19	+0·14	+0·10	+0·27	+0·39	+0·30	+0·44	+0·26	+0·46	+0·46	+0·29	+0·27	+0·44	+0·17		+0·11	+0·09	+0·05	+0·13	S.W.*C.*	S.W.*S.*	S.	N.W.
21.	6																							N.E.*C.*	S.W.*S.*	S.W.*C.*	N.W.
21.	18	−0·11	−0·13	−0·14	−0·07	−0·16	−0·19	−0·62	−0·52	−0·07	−0·12	−0·14	−0·14	−0·05	−0·18	−0·15	−0·41	−0·10	−0·06	−0·06	−0·15	−0·10	−0·02	N.W.*C.*	N.W.	N.E.*C.*	N.W.
22.	6																							E.*C.*	S.W.	W.*C.*	N.W.
22.	19	−0·41	−0·44	−0·45	−0·47	−0·44	−0·50	−0·13	−0·24	−0·29	−0·36	−0·41	−0·47	−0·49	−0·58	−0·57	−0·50	−0·63		+0·37	−0·56	−0·44	−0·26	N.E.*C.*	E.N.E.*C.*	S.E.*C.*	N.W.
23.	7	−0·07	−0·02	−0·08	−0·02	−0·07	−0·07	−0·11	−0·27	+0·27	+0·17	+0·03	+0·05	+0·02	−0·10	−0·10	−0·10	−0·05		+0·17	+0·10	+0·08	+0·03	S.W.*C.*	S.E.	N.E.*C.*	N.N.W.
																								S.W.	W.S.W.	E.*C.*	W.

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Upon inspecting these numbers, one law cannot fail to occur to us, namely, that the irregularities are nearly the same in magnitude and in sign on every part of the coast of Ireland at the same time. So prevalent is this law, that there are few instances in which the irregularity at one place differs from the mean of the irregularities at all the places at the same time by more than an inch. My ideas of the almost perfect fluidity of water have been very much raised by this comparison. I may remark that it embodies, in a form admitting of easy examination, the result concluded from rough inspection of observations which is made the foundation of a method for supplying deficient observations described in page 10. I may also add that it gives no small security for the general fidelity and accuracy of the observers at the different stations.

I do not perceive any certain connexion between the irregularities and the course of the winds, except that the water is usually highest on all parts of the coast with a violent south-west wind.

In order to ascertain the general relation of these irregularities to those of the barometer, I have compared the corrected mean height of the barometer at Greenwich for each civil day with the mean height for the year (using the numbers published in the Greenwich Magnetical and Meteorological Observations). The following are the results:—

Excess of the corrected mean height of the barometer each day, at Greenwich, above the mean height for the year 1842.

	in.		in.		in.		in.		in.		in.
June 23.	−0·142	July 4.	−0·226	July 15.	+0·378	July 26.	+0·042	Aug. 6.	−0·033	Aug. 17.	+0·132
24.	−0·251	5.	−0·232	16.	+0·174	27.	+0·187	7.	....	18.	−0·041
25.	−0·234	6.	+0·129	17.	....	28.	+0·056	8.	+0·078	19.	−0·067
26.	....	7.	+0·070	18.	−0·118	29.	−0·069	9.	+0·017	20.	+0·032
27.	+0·218	8.	−0·216	19.	−0·147	30.	+0·083	10.	−0·263	21.	....
28.	+0·268	9.	−0·271	20.	−0·231	31.	....	11.	−0·029	22.	−0·023
29.	+0·097	10.	....	21.	−0·189	Aug. 1.	+0·310	12.	+0·313	23.	−0·064
30.	−0·080	11.	−0·288	22.	+0·064	2.	+0·164	13.	+0·443		
July 1.	−0·168	12.	−0·046	23.	+0·235	3.	−0·016	14.	....		
2.	−0·105	13.	+0·226	24.	....	4.	−0·092	15.	+0·242		
3.	....	14.	+0·388	25.	−0·040	5.	−0·036	16.	+0·207		

I have also obtained, through the kindness of Sir W. R. Hamilton, the barometrical observations at the Observatory of Dunsink, near Dublin; and from Colonel Colby and Captain Larcom, R.E., I have received the observations made in the Phœnix Park near Dublin, and at Limerick. The following results are obtained by comparing each day's mean with the mean for this period.

Excess of the mean height of the barometer each day, at Dublin, above the mean of all.

	Dunsink.	Phœnix Park.		Dunsink.	Phœnix Park.		Dunsink.	Phœnix Park.		Dunsink.	Phœnix Park.		Dunsink.	Phœnix Park.		Dunsink.	Phœnix Park.
	in.	in.		in.	in.		in.	in.		in.	in.		in.	in.		in.	in.
June 23.	−0·31	−0·372	July 4.	−0·53	−0·547	July 15.	+0·35	+0·388	July 26.	+0·19	+0·164	Aug. 6.	−0·04	−0·070	Aug 17.	+0·09	+0·059
24.	−0·42	−0·454	5.	−0·22	−0·281	16.	+0·04	+0·076	27.	+0·31	+0·302	7.	.....	−0·112	18.	+0·01	−0·013
25.	−0·49	−0·534	6.	+0·06	+0·173	17.	.....	−0·196	28.	+0·20	+0·182	8.	−0·08	−0·097	19.	−0·17	−0·210
26.	+0·06	−0·098	7.	−0·22	−0·227	18.	−0·13	−0·153	29.	+0·20	+0·173	9.	−0·10	−0·111	20.	−0·08	−0·105
27.	+0·31	+0·315	8.	−0·38	−0·500	19.	−0·12	−0·165	30.	+0·29	+0·284	10.	−0·21	−0·258	21.	+0·12	+0·115
28.	+0·16	+0·148	9.	−0·32	−0·365	20.	−0·11	−0·147	31.	.....	+0·365	11.	−0·01	−0·011	22.	+0·17	+0·165
29.	+0·13	+0·120	10.	−0·25	−0·180	21.	+0·01	−0·006	Aug. 1.	+0·32	+0·315	12.	+0·17	+0·170	23.	−0·07	−0·079
30.	+0·02	+0·022	11.	−0·43	−0·485	22.	+0·22	+0·205	2.	+0·09	+0·072	13.	+0·42	+0·418			
July 1.	−0·10	−0·126	12.	−0·09	−0·074	23.	+0·30	+0·304	3.	+0·10	+0·089	14.	.....	+0·445			
2.	−0·04	−0·059	13.	+0·14	+0·121	24.	.....	+0·200	4.	−0·09	−0·110	15.	+0·37	+0·342			
3.	−0·25	−0·108	14.	+0·45	+0·443	25.	+0·09	+0·061	5.	−0·14	−0·176	16.	+0·29	+0·284			

Excess of the mean height of the barometer each day, at Limerick, above the mean of all.											
	in.		in.		in.		in.		in.		in.
June 23.	−0·407	July 4.	−0·543	July 15.	+0·365	July 26.	+0·182	Aug. 6.	−0·062	Aug. 17.	+0·058
24.	−0·447	5.	−0·169	16.	−0·059	27.	+0·324	7.	−0·169	18.	−0·062
25.	−0·535	6.	+0·212	17.	−0·320	28.	+0·295	8.	−0·171	19.	−0·198
26.	−0·077	7.	−0·217	18.	−0·208	29.	+0·220	9.	−0·152	20.	−0·115
27.	+0·360	8.	−0·514	19.	−0·178	30.	+0·337	10.	−0·267	21.	+0·099
28.	+0·178	9.	−0·346	20.	−0·132	31.	+0·381	11.	−0·053	22.	+0·190
29.	+0·178	10.	−0·191	21.	+0·030	Aug. 1.	+0·323	12.	+0·122	23.	−0·069
30.	+0·039	11.	−0·491	22.	+0·173	2.	+0·052	13.	+0·406		
July 1.	−0·072	12.	−0·104	23.	+0·308	3.	+0·124	14.	+0·422		
2.	+0·022	13.	+0·094	24.	+0·206	4.	−0·051	15.	+0·360		
3.	−0·116	14.	+0·440	25.	+0·074	5.	−0·162	16.	+0·301		

The comparison of these numbers with the irregularities in the heights of the water amply supports the law of DAUSSY, WHEWELL, and BUNT, that a negative irregularity in the height of the barometer is accompanied by a positive irregularity in the height of the sea, twelve or fourteen times as great as that of the barometer.

Section XII.—*Discussion of range of tide, or coefficient of first arc in the analysis of individual tides; and of semimenstrual inequality in range, apparent proportion of solar and lunar effects, and age of tide as deduced from range.*

The tides were divided into groups of large tides and small tides, separated at the same times as those particularized in pages 29 and 96. For each of these groups the mean of the values of C_1 was taken, some deficient values being supplied by interpolation. The moon's parallax and the square of the cosine of the moon's and sun's declinations were taken for two days preceding each tide, and the means of these quantities were taken through the same groups.

A general result will be obtained by forming the sum of the mean values of C_1 for all the stations in each group. Thus we obtain:—

Large Tides.				
Period.	Sum of mean values of C_1 at all the stations.	Mean value of $\cos^2$ moon's declination.	Mean value of moon's parallax.	Mean value of $\cos^2$ sun's declination.
h h	ft.		′ ″	
July 6. 16 to July 13. 3	117·43	0·8742	59 44	0·8547
July 20. 9 to July 28. 9	100·11	0·9076	54 41	0·8815
Aug. 5. 0 to Aug. 11. 16	129·11	0·9260	60 31	0·9194

Small Tides.				
Period.	Sum of mean values of C_1 at all the stations.	Mean value of $\cos^2$ moon's declination.	Mean value of moon's parallax.	Mean value of $\cos^2$ sun's declination.
h h	ft.		′ ″	
June 28. 6 to July 6. 16	71·66	0·9427	55 13	0·8456
July 13. 3 to July 20. 9	84·63	0·9202	58 19	0·8666
July 28. 9 to Aug. 5. 0	73·40	0·8965	55 56	0·9012
Aug. 11. 16 to Aug. 18. 15	77·84	0·8697	57 34	0·9378

Put M for the lunar effect when the square of the cosine of the moon's declination is 0·9 and her parallax is 57′; m for the quantity by which this is increased for every increase of 1′ in parallax; s for the mean effect of the sun (the square of the cosine of his declination being 0·9) for one-fourth of a lunation, positive in the large tides and negative in the small tides, which bears to the absolute effect of the sun a relation explained in page 34. The effect of the variations of the moon's parallax and declination upon the luni-solar tide, as is well known, is nearly the same as that on the simple lunar tide; and therefore it will be correct to refer the mean of the luni-solar tides to the mean of the moon's parallaxes and square of cosine of declinations. The variations depending on the moon's declinations are not strictly in the proportion of the squares of the cosines of her declination, but in the present instance, where the means of the squares of the cosines are very nearly equal, may be assumed to be so without sensible error. Forming then an equation from each of the lines in the Table above by these considerations; reducing them to four equations by retaining the second, taking the mean of the first and third, the mean of the fourth and sixth, and the mean of the fifth and seventh, and combining these so as to form three favourable equations, by adding all, by subtracting the sum of the third and fourth from the sum of the others, and by subtracting the sum of the second and third from the sum of the others, we obtain the following equations:—

$$\begin{aligned} 377{\cdot}15 &= \mathrm{M}\times 4{\cdot}024 + m\times 0{\cdot}33 - s\times 0{\cdot}008 \\ 69{\cdot}61 &= -\mathrm{M}\times 0{\cdot}008 + m\times 1{\cdot}29 + s\times 3{\cdot}938 \\ 31{\cdot}87 &= -\mathrm{M}\times 0{\cdot}036 + m\times 7{\cdot}83 - s\times 0{\cdot}026. \end{aligned}$$

From these we obtain M=93·4, m=4·56, s=16·37. The moon's effect, therefore, for the parallax 57′+n' may be represented by $93{\cdot}4+4{\cdot}56\times n$. If the moon's hydrodynamical effect varied as the cube of her parallax (which is the law of variation of her statical effect), the formula would be $93{\cdot}4+4{\cdot}92\times n$. The result of the movement of the water has therefore been, to reduce the elliptic variation of lunar effect by $\frac{0{\cdot}36}{4{\cdot}92}$ part, or by $\frac{3}{41}$ part.

Now it is shown in the Encyclopædia Metropolitana, *Tides and Waves*, Art. 448, that if the tides were created by the effect of the moon on the water in a uniform channel surrounding the earth, and if b were the earth's radius, k the depth of the water, g the acceleration produced by gravity in the unit of time, n' the moon's apparent angular motion round the earth (as estimated by a spectator who supposes that the earth does not revolve on an axis), and h the moon's angular motion from her perigee; then the elliptic variation is changed by $\frac{4}{3}\cdot\frac{n'b^2h}{n'^2b^2-gk}$ part. Thus we obtain

$$\frac{4}{3}\cdot\frac{n'b^2h}{n'^2b^2-gk}=-\frac{3}{41}.$$

Making $\frac{h}{n}=\frac{3}{80}$, we find $\frac{gk}{n'^2b^2}=\frac{101}{60}=\frac{5}{3}$ nearly; and $k=\frac{5}{3}\cdot\frac{n'^2b^2}{g}$. Observing that $n'b$

$=$linear velocity of a point at the equator produced by the earth's rotation, supposing the moon fixed, we easily find $\frac{n'^2b^2}{g}=13$ miles nearly; and thus our observations give

Depth of the sea $=k=22$ miles.

If the channel were supposed to be a small circle of the earth instead of a large one, the resulting depth of the sea would be diminished in the proportion of the square of its diameter.

Whatever may be supposed of the error of this result, or the inapplicability of the theory by which it is obtained to the circumstances of the seas, I may remark that it agrees generally with a result deduced from Mr. WHEWELL's discussions of the observations at Bristol with reference to the moon's declinations*.

Assuming however that we have correctly determined $\frac{n'^2b^2}{gk}=\frac{3}{5}$, we may proceed to remark that† the moon's hydrodynamical effect is represented by her statical effect multiplied by $\frac{1}{gk-n'^2b^2}$ and by constants; and that the sun's hydrodynamical effect is represented by his statical effect multiplied by $\frac{1}{gk-n^2b^2}$ and by the same constants. If we consider $\frac{n^2}{n'^2}=\frac{15}{14}$, then the hydrodynamical effect of the moon contains the multiplier

$$\frac{1}{gk}\cdot\frac{1}{1-\frac{3}{5}}=\frac{1}{gk}\cdot\frac{5}{2}, \text{ or } \frac{1}{gk}\cdot\frac{25}{10},$$

while that for the sun contains the multiplier

$$\frac{1}{gk}\cdot\frac{1}{1-\frac{15}{14}\cdot\frac{3}{5}}=\frac{1}{gk}\cdot\frac{14}{5} \text{ or } \frac{1}{gk}\cdot\frac{28}{10}.$$

And therefore the proportion of the moon's statical effect to the sun's is greater than the proportion of her dynamical effect to the sun's in the ratio of 28 to 25. And as the moon's hydrodynamical effect, deduced from the values of M and s above (93·4 and 16·37), by the considerations in page 34, is nearly $=\frac{1\cdot00}{0\cdot28}\times$ sun's hydrodynamical effect, it follows that the moon's statical effect $=\frac{1\cdot00}{0\cdot25}\times$ sun's statical effect $=4\times$ sun's statical effect. This conclusion differs widely from LAPLACE's; yet it is formed, as I believe, on grounds as good as LAPLACE's.

For particular results applying to each individual station, regarding the semi-menstrual inequality in range and the apparent proportion of the solar and lunar hydrodynamical effects; the mean value of C_1 for large tides is found by taking the mean of the three values in the three periods of the last Table, and the mean value for small tides by taking the mean of the four values in the four periods of the last

* Tides and Waves, Art. 553. † Ibid. Art. 448.

Table; and then treating these in the same manner as in the latter part of the table on page 35.

Station.	Mean of C_1 for large tides.	Mean of C_1 for small tides.	Difference.	Mean, or M.	Difference divided by mean.	Corresponding value of $\frac{S}{M}$.
	ft.	ft.	ft.	ft.		
Kilbaha	6·12	3·73	2·39	4·93	0·48	0·40
Kilrush	6·54	4·19	2·35	5·37	0·44	0·36
Foynes Island....	7·35	4·96	2·39	6·16	0·39	0·32
Limerick.....	8·98	6·21	2·77	7·59	0·37	0·30
Casleh Bay.....	6·35	3·83	2·52	5·09	0·50	0·42
Galway	6·41	3·89	2·52	5·15	0·49	0·41
Old Head	5·59	3·43	2·16	4·51	0·48	0·40
Mullaghmore	5·35	3·25	2·10	4·30	0·49	0·41
Buncrana	5·84	3·37	2·47	4·61	0·54	0·46
Port Rush	2·56	1·38	1·18	1·97	0·60	0·52
Carrowkeel.....	3·35	1·95	1·40	2·65	0·53	0·45
Ballycastle	1·42	0·88	0·54	1·15	0·47	0·39
Glenarm.....	2·89	2·47	0·42	2·68	0·16	0·12
Donaghadee	5·24	3·96	1·28	4·60	0·28	0·22
Ardglass.....	6·90	4·96	1·94	5·93	0·33	0·26
Clogher Head ..	6·79	4·81	1·98	5·80	0·34	0·27
Kingstown	5·26	3·69	1·57	4·48	0·35	0·28
Dunmore East ..	5·73	3·91	1·82	4·82	0·38	0·31
New Ross	6·16	4·51	1·65	5·34	0·31	0·25
Passage West....	5·94	4·19	1·75	5·07	0·35	0·28
Castle Townsend..	4·79	3·33	1·46	4·06	0·36	0·29

For the age of tide as inferred from range, the times have been ascertained (by interpolating between the times in the Tables of formulæ in Section X.) at which the actual value of C_1 may be supposed to coincide with the mean value of C_1; and the times thus found have been compared with the times at which the moon's hour-angle from the sun was 3^h, 9^h, 15^h, 21^h, namely, June 26, 6^h, July 4, 16^h, July 11, 3^h, July 18, 9^h, July 26, 9^h, August 3, 0^h, August 9, 16^h, and August 16, 15^h. An error is here committed alternately + and −, and therefore it is proper to use an even number of comparisons. Eight are used at every place except Ballycastle and Glenarm, where only six are used. [It is to be remarked that in the use of the formulæ in Section X., a number opposite to a bracket is always held to correspond to the mean of the two times embraced by that bracket.] The means of all the differences at each station between the times thus found from Section X. and the times corresponding to the hour-angles 3^h, 9^h, &c., are adopted in the following Table as the age of the tide. These* are the true ages of the tide.

Station.	Age of tide.	Station.	Age of tide.	Station.	Age of tide.
	d h		d h		d h
Kilbaha	1 20	Mullaghmore....	2 0	Ardglass.....	2 2
Kilrush	1 19	Buncrana	1 20	Clogher Head ..	2 2
Foynes Island....	1 21	Port Rush	1 11	Kingstown	2 0
Limerick.....	2 1	Carrowkeel.....	1 14	Dunmore East ..	2 2
Casleh Bay.....	1 20	Ballycastle	1 5	New Ross	2 2
Galway	1 21	Glenarm.....	2 9	Passage West....	2 1
Old Head	1 22	Donaghadee	2 6	Castle Townsend..	1 23

* Tides and Waves, Art. 545.

Section XIII.—*Establishment of each port, as deduced from the time of maximum of the first periodical term in the analysis of individual tides.*

The preceding operations having made it sufficiently clear that the age of the tide differs little from two days, it is proper now to refer all the phenomena of the tide to an epoch two days earlier than the observation. A process is therefore adopted in this section differing in a trifling degree from that in Section VII. The times are taken at which the moon's hour-angle from the sun was 0^h, 6^h, 12^h, 18^h, and two days are added to these times: the resulting times are June 24, 9^h, July 2, 20^h, July 9, 19^h, July 16, 21^h, July 23, 23^h, August 1, 16^h, August 8, 4^h, August 15, 5^h, August 22, 16^h. These times are assumed to divide the large lunitidal intervals from the small ones. For the lunitidal intervals at the high waters of the First Division, the moon's time of Greenwich transit is taken for that transit which precedes the high water by two days and a few hours; for the high waters of the Second Division the moon's time of Greenwich lower transit is interpolated. Comparing these times of transit with the times of high water in the formulæ of Section X., the lunitidal intervals corresponding to lunar transits two days earlier are found. The means of these are taken for the groups separated by the times specified above. Then, the means of the large and the small lunitidal intervals being adopted as the mean interval, a correction of $1^h\ 41^m$ is applied subtractively, to reduce the interval after moon's transit two days previous, to the interval after moon's transit on the same day as the tide ($1^h\ 41^m$ being the difference between two solar days and two lunar days). The result is contained in the following Table.

Station.	Mean of large intervals from transit two days previous.	Mean of small intervals from transit two days previous.	Mean of all the intervals from transit two days previous.	True establishment in Greenwich time.
	h m	h m	h m	h m
Kilbaha	6 53	5 55	6 24	4 43
Kilrush	7 6	6 7	6 36	4 55
Foynes Island....	7 49	6 54	7 21	5 40
Limerick.....	8 19	7 27	7 53	6 12
Casleh Bay.....	7 3	6 3	6 33	4 52
Galway	7 8	6 9	6 38	4 57
Old Head	7 19	6 23	6 51	5 10
Mullaghmore	7 39	6 37	7 8	5 27
Buncrana	8 27	7 21	7 54	6 13
Port Rush	9 4	7 50	8 27	6 46
Carrowkeel.....	9 48	8 44	9 16	7 35
Ballycastle	10 4	9 2	9 33	7 52
Glenarm.....	12 53	12 3	12 28	10 47
Donaghadee	13 10	12 16	12 43	11 2
Ardglass.....	13 14	12 16	12 45	11 4
Clogher Head ..	13 21	12 24	12 52	11 11
Kingstown	13 26	12 30	12 58	11 17
Dunmore East ..	7 14	6 16	6 45	5 4
New Ross	8 5	7 11	7 38	5 57
Passage West....	7 26	6 31	6 58	5 17
Castle Townsend..	7 3	6 5	6 34	4 53

Section XIV.—*Semimenstrual inequality in time, proportion of solar and lunar effects from times, and apparent age of tide as shown by times; deduced from the time of maximum of the first periodical term in the analysis of individual tides.*

Taking the difference of the means of large intervals and small intervals in the last Table, we deduce from them the value of $\frac{S}{M}$ by the same formula as that employed in page 42. The results are as follows:—

Station.	Difference of means of large intervals and small intervals.	Value of $\frac{S}{M}$.	Station.	Difference of means of large intervals and small intervals.	Value of $\frac{S}{M}$.
	m			m	
Kilbaha	58	0·37	Ballycastle	62	0·40
Kilrush	59	0·38	Glenarm	50	0·33
Foynes Island....	55	0·36	Donaghadee	54	0·35
Limerick.....	52	0·34	Ardglass.....	58	0·37
Casleh Bay.....	60	0·39	Clogher Head ..	57	0·37
Galway	59	0·38	Kingstown	56	0·36
Old Head	56	0·36	Dunmore East ..	58	0·37
Mullaghmore	62	0·40	New Ross	54	0·35
Buncrana	66	0·42	Passage West....	55	0·36
Port Rush	74	0·48	Castle Townsend..	58	0·37
Carrowkeel.....	64	0·41			

These are the true values of $\frac{S}{M}$*, supposing that the process for finding them has been correctly followed. I omit here all deductions as to the absolute maximum of semimenstrual inequality in time at each station, for a reason that will be explained in Section XV.

To obtain the apparent age of tide as shown by times, a process was used analogous to that in page 43 or that in page 106. The times were ascertained (by interpolating between the times in the Tables of formulæ in Section X.) at which the actual interval of analysed high water from moon's transit two days previous coincided with the mean interval in the Table of page 107. These times were then compared with the times at which the moon's hour-angle from the sun was 0^h, 6^h, 12^h, 18^h; namely, June 22, 9^h, June 30, 20^h, July 7, 19^h, July 14, 21^h, July 21, 23^h, July 30, 16^h, August 6, 4^h, August 13, 5^h, August 20, 16^h. The difference was considered to be the apparent age of the tide as given by the times. The following are the results:—

Station.	Apparent age of tide from times.	Station.	Apparent age of tide from times.	Station.	Apparent age of tide from times.
	d h		d h		d h
Kilbaha	1 7	Mullaghmore	0 23	Ardglass.....	1 11
Kilrush	1 10	Buncrana	1 4	Clogher Head....	1 11
Foynes Island....	1 22	Port Rush	0 9	Kingstown	1 3
Limerick.....	2 7	Carrowkeel.....	+1 0	Dunmore East ..	2 7
Casleh Bay.....	1 4	Ballycastle	−0 14	New Ross	2 18
Galway	1 3	Glenarm	+1 12	Passage West....	1 23
Old Head	1 6	Donaghadee	1 13	Castle Townsend..	1 11

* Tides and Waves, Art. 538.

The apparent age from times is at every littoral station, except Dunmore, considerably less than the age from range. At Port Rush the apparent age is small; and at Ballycastle, the diminution proceeds so far as to change the sign of the apparent age. I cannot entirely explain this difference. It indicates that large tides arrive earlier (with reference to the hour-angles of the sun and moon) than small ones; but I know not why this should happen.

Section XV.—*Comparison of the results as to mean height, range, semimenstrual inequality in height, age of tide obtained from height, establishment, semimenstrual inequality in time, and age of tide obtained from times, deduced from high and low waters only, in* Sections V., VI., VII., VIII., *with those deduced from the analysis of individual tides in* Sections XI., XII., XIII., XIV.

With regard to the mean heights, we have to compare the results in Section V. with those in Section XI. And first for the mean height on the whole series of observations. The stations which appear best adapted to enable us to decide on the adoption of Mean Heights or Apparent Mean Heights as our standard (that is, as most nearly related to the height of water unaffected by tides) are those upon the Shannon. And these leave no doubt that the Mean Heights (deduced from the analysis) ought to be adopted. In a current river, it is inconceivable that the height at a lower station (as Foynes Island) should be equal to that at the highest station (Limerick), as it would be if we relied on apparent mean heights. These are the only stations which throw much light on this subject, for at Dunmore East and New Ross (river stations) the two results agree; and, at the littoral stations, there is on the whole no difference of a critical kind. At the two quasi-river-stations of Buncrana and Carrowkeel, we have on both systems discordant results, one giving a mean level higher and the other lower than that of the more exposed stations. The extreme diminution of the range of tide between Port Rush and Glenarm causes no sensible alteration of the mean level, in either system of results. The greater elevation of the mean level at the northern part of the island is equally well-marked in both. For the variation of mean height under different circumstances of large and small tides, large and small declinations of the moon, and increasing and decreasing declinations of the moon, the comparison of the numbers on the two systems gives little subject for remark, except that the difference between large and small tides in the Shannon stations seems to be more strongly marked in the mean heights than in the apparent mean heights. This however does not well accord with the idea of a standard height. It will be remarked that at Limerick the difference between the mean height and the apparent mean height is nearly a foot.

With regard to the range of the tide, we have to compare the "Mean Range" in the 8th column of the Table in page 35 with the double of "Mean or M" in the 5th column of the Table on page 106. Neglecting the differences of 0·01, we may assert that, at all the stations except Mullaghmore and Dunmore East, the apparent range

is greater than 2 C_1 in the formulæ of Section X.; and that in the river-stations the difference is considerable. Without accounting for the two exceptions, I may remark that this shows that the departure from the pure form of tide depending on a single sine is, at all the other stations, similar in some important points of its character to that in a river tide. The nature of the tide at each station will be examined more accurately in Section XVI.

With regard to the semimenstrual inequality in height and the apparent value of $\frac{S}{M}$, we have to compare the two last columns of the Table on page 105 with the two similar columns of the Table on page 35. The numbers are so nearly equal that we cannot assert that there is any certain difference. The large value of $\frac{S}{M}$ at Port Rush, the small value at Glenarm, and the general smallness from Donaghadee to Castle Townsend (like that in the river station Limerick) are equally marked in both.

With regard to the age of the tide as obtained from heights or ranges (which, as I have stated before, is the true age of the tide), we must compare the results on page 105 with those on page 38. They agree, on the whole, very well; though the ages deduced from the analysis appear to be somewhat smaller than those deduced from high and low waters. The ages deduced from the analysis also agree better among themselves. The diminution however from Port Rush to Ballycastle is remarkable. The general result seems to be that on the south-western coast of Ireland the age of the tide is about one day twenty hours.

For the establishment, we must compare the Table of page 106 with that of page 39. Only at Limerick and New Ross is the difference considerable: at these stations it amounts to about twenty minutes.

For the semimenstrual inequality in time, we must compare the numbers in page 107 with those of page 42. And here it will at once be remarked that a great and important change has been made in the resulting values of $\frac{S}{M}$ by the new mode of treating them. The values at Old Head and Glenarm and the following stations are increased, and those at Port Rush and Ballycastle are diminished, till they agree sufficiently well with the others. This change arises from two causes. First, the determination of times from the analysis is vastly more accurate than that from the estimation of the times of high and low water. Secondly, in Section VIII. the groups for large intervals and small intervals were divided at each station from a consideration of the magnitudes of the intervals themselves, whereas in Section XIV. they were divided from consideration of a totally different circumstance, namely the age of the tide as shown by the time of occurrence of the mean value of range. Strange as it may appear, the former method was incorrect, and the latter is correct. The former method is affected by a circumstance which ought not to enter into the formation of this result at all, namely the change in the time of station-tide, not depending on the change in the time of sea-tide, but depending on the change which the character of the tidal for-

mula in Section X. undergoes when the magnitude of the range is altered. Thus, the maximum and minimum intervals of sea-tide (which are the objects of our search) occur at the times (corrected for age of tide) when the moon's hour-angle from the sun is 3^h, 9^h, &c. But from hour-angle 9^h to hour-angle 12^h the range of tide is increasing: the modification of time of station-tide as related to time of sea-tide is therefore increasing; a second inequality of time is therefore combined with the first, having a different time of vanishing: the time of vanishing of the compound inequality is therefore different from that of the first inequality, and the maximum magnitude of the compound inequality is different from that of the first. And this produces its full effect if we make our divisions of groups with reference to the time of vanishing of the compound inequality. But if we make our divisions strictly at the times when the first inequality ought to vanish, then, though every individual time be affected by the second inequality, yet there are the same number of instances affected in the same way on opposite sides of our places of division, and their effect disappears in the final result. But as the full compound effect is not used for our final result, conversely we cannot from our final result infer the maximum magnitude of the full compound effect, or the maximum value of the semimenstrual inequality in time.

These considerations appear to be deserving of the utmost attention in investigating the most important single result which can be deduced from the tides, namely the proportion of the hydrodynamical effects of the sun and the moon.

The mean of all the values of $\frac{S}{M}$ in page 108 is 0·38. It is probable that, on attending more scrupulously to the age of the tide at the different stations, results would have been found agreeing more closely with each other: but I think it likely that the mean would scarcely have been altered.

With regard to the apparent age of tide obtained from times, we must compare the numbers in page 108 with those in page 44. The general result of the comparison is, that the mean of the opposite ages deduced from high and low water agrees with that deduced from the analysis as nearly as can be expected (where a small error in the estimated time of high or low water would produce a great effect on the resulting age). They agree in giving a small apparent age at Port Rush and Ballycastle. The ages deduced from range agree also in this. Now this result* is exactly that which would follow from the supposition that a tidal wave with a large range travels more quickly over the shallow bottom than one with a small range: and that this holds not for the phase of high water or low water only, but for the zero of the angle p. Or it would follow from the supposition that a wave of short period travels more quickly than one of long period (the tidal day near conjunction being shorter than that near quadrature). Neither of these points has been established by theory; but the former appears to be very probable.

* Tides and Waves, Art. 463.

Section XVI.—*Remarks on the succeeding terms of the expressions for individual tides, as related to the magnitude of the tide, to the position on the sea-coast, to the position on the river, &c.; comparison with the terms given by the theory of waves; discussion of the quarto-diurnal tide.*

In order to reduce to a smaller number the numerous formulæ of Section X., and to render the relations of their coefficients and arguments at once accurate and distinct, the formulæ have been divided into groups corresponding to large tides and small tides, the times of division being the same as those used in the discussions of the semimenstrual inequality of height. Then, for the large tides, the numbers $\frac{32}{\pi} A_2$ have been collected and their mean has been taken; similarly, the mean of the numbers $\frac{32}{\pi} B_2$ has been taken; and from these, by the treatment described in Section X., a term similar to $C_2 \sin(2 \text{ phase} + \varphi_2)$ is formed. The mean of all the angles φ_1 is also taken, and its double is subtracted from the number corresponding to φ_2. A similar process is used for 3 phase and 4 phase. As for some tides φ_1 is nearly equal to 90° and for others is nearly equal to 270°; the expressions in the latter case are made to admit of combination with those in the former case by subtracting 180° from φ_1 and by changing the signs of A_3 and B_3. The small tides are treated in the same manner. The following Table contains the result.

Variable part of the formulæ for the elevation of the water at each of the Stations in Large Tides and Small Tides.

		ft.	ft.	ft.	ft.
Kilbaha	Large tides.	$6{\cdot}12.\sin p$	$+0{\cdot}16.\sin(2p+164^\circ)$	$+0{\cdot}03.\sin(3p+231^\circ)$	$+0{\cdot}03.\sin(4p+88^\circ)$
	Small tides.	$3{\cdot}73.\sin p$	$+0{\cdot}08.\sin(2p+208)$	$+0{\cdot}06.\sin(3p+248)$	$+0{\cdot}01.\sin(4p+120)$
Kilrush	Large tides.	$6{\cdot}54.\sin p$	$+0{\cdot}19.\sin(2p+104)$	$+0{\cdot}06.\sin(3p+195)$	$+0{\cdot}03.\sin(4p+167)$
	Small tides.	$4{\cdot}19.\sin p$	$+0{\cdot}12.\sin(2p+128)$	$+0{\cdot}06.\sin(3p+210)$	$+0{\cdot}02.\sin(4p+175)$
Foynes Island	Large tides.	$7{\cdot}35.\sin p$	$+0{\cdot}55.\sin(2p+111)$	$+0{\cdot}26.\sin(3p+162)$	$+0{\cdot}03.\sin(4p+277)$
	Small tides.	$4{\cdot}96.\sin p$	$+0{\cdot}37.\sin(2p+124)$	$+0{\cdot}11.\sin(3p+175)$	$+0{\cdot}01.\sin(4p+327)$
Limerick	Large tides.	$8{\cdot}98.\sin p$	$+0{\cdot}90.\sin(2p+75)$	$+0{\cdot}68.\sin(3p+125)$	$+0{\cdot}38.\sin(4p+160)$
	Small tides.	$6{\cdot}21.\sin p$	$+0{\cdot}74.\sin(2p+111)$	$+0{\cdot}40.\sin(3p+145)$	$+0{\cdot}15.\sin(4p+182)$
Casleh Bay	Large tides.	$6{\cdot}35.\sin p$	$+0{\cdot}16.\sin(2p+165)$	$+0{\cdot}12.\sin(3p+214)$	$+0{\cdot}06.\sin(4p+348)$
	Small tides.	$3{\cdot}83.\sin p$	$+0{\cdot}07.\sin(2p+219)$	$+0{\cdot}07.\sin(3p+238)$	$+0{\cdot}03.\sin(4p+35)$
Galway	Large tides.	$6{\cdot}41.\sin p$	$+0{\cdot}18.\sin(2p+157)$	$+0{\cdot}18.\sin(3p+204)$	$+0{\cdot}16.\sin(4p+349)$
	Small tides.	$3{\cdot}89.\sin p$	$+0{\cdot}08.\sin(2p+204)$	$+0{\cdot}12.\sin(3p+233)$	$+0{\cdot}04.\sin(4p+38)$
Old Head	Large tides.	$5{\cdot}59.\sin p$	$+0{\cdot}08.\sin(2p+154)$	$+0{\cdot}04.\sin(3p+196)$	$+0{\cdot}04.\sin(4p+282)$
	Small tides.	$3{\cdot}43.\sin p$	$+0{\cdot}04.\sin(2p+216)$	$+0{\cdot}10.\sin(3p+257)$	$+0{\cdot}01.\sin(4p+237)$
Mullaghmore	Large tides.	$5{\cdot}31.\sin p$	$+0{\cdot}06.\sin(2p+160)$	$+0{\cdot}07.\sin(3p+14)$	$+0{\cdot}06.\sin(4p+184)$
	Small tides.	$3{\cdot}25.\sin p$	$+0{\cdot}04.\sin(2p+217)$	$+0{\cdot}03.\sin(3p+134)$	$+0{\cdot}02.\sin(4p+254)$
Buncrana	Large tides.	$5{\cdot}84.\sin p$	$+0{\cdot}14.\sin(2p+165)$	$+0{\cdot}08.\sin(3p+262)$	$+0{\cdot}04.\sin(4p+295)$
	Small tides.	$3{\cdot}37.\sin p$	$+0{\cdot}04.\sin(2p+199)$	$+0{\cdot}04.\sin(3p+256)$	$+0{\cdot}01.\sin(4p+237)$
Port Rush	Large tides.	$2.56.\sin p$	$+0{\cdot}08.\sin(2p+153)$	$+0{\cdot}12.\sin(3p+257)$	$+0{\cdot}01.\sin(4p+207)$
	Small tides.	$1.38.\sin p$	$+0{\cdot}04.\sin(2p+220)$	$+0{\cdot}07.\sin(3p+287)$	$+0{\cdot}02.\sin(4p+172)$
Carrowkeel	Large tides.	$3{\cdot}35.\sin p$	$+0{\cdot}07.\sin(2p+238)$	$+0{\cdot}11.\sin(3p+205)$	
	Small tides.	$1{\cdot}95.\sin p$	$+0{\cdot}01.\sin(2p+319)$	$+0{\cdot}06.\sin(3p+199)$	$+0{\cdot}01.\sin(4p+152)$
Ballycastle	Large tides.	$1{\cdot}42.\sin p$	$+0{\cdot}07.\sin(2p+308)$	$+0{\cdot}07.\sin(3p+276)$	$+0{\cdot}03.\sin(4p+218)$
	Small tides.	$0{\cdot}88.\sin p$	$+0{\cdot}06.\sin(2p+338)$	$+0{\cdot}03.\sin(3p+311)$	$+0{\cdot}01.\sin(4p+293)$
Glenarm	Large tides.	$2{\cdot}89.\sin p$	$+0{\cdot}17.\sin(2p+76)$	$+0{\cdot}07.\sin(3p+227)$	$+0{\cdot}01.\sin(4p+310)$
	Small tides.	$2{\cdot}47.\sin p$	$+0{\cdot}05.\sin(2p+50)$	$+0{\cdot}04.\sin(3p+237)$	
Donaghadee	Large tides.	$5{\cdot}24.\sin p$	$+0{\cdot}07.\sin(2p+56)$	$+0{\cdot}03.\sin(3p+174)$	$+0{\cdot}01.\sin(4p+327)$
	Small tides.	$3{\cdot}96.\sin p$	$+0{\cdot}03.\sin(2p+318)$	$+0{\cdot}03.\sin(3p+230)$	$+0{\cdot}01.\sin(4p+85)$
Ardglass	Large tides.	$6{\cdot}90.\sin p$	$+0{\cdot}18.\sin(2p+98)$	$+0{\cdot}04.\sin(3p+145)$	$+0{\cdot}03.\sin(4p+125)$
	Small tides.	$4{\cdot}96.\sin p$	$+0{\cdot}07.\sin(2p+117)$	$+0{\cdot}04.\sin(3p+217)$	$+0{\cdot}03.\sin(4p+161)$
Clogher Head	Large tides.	$6{\cdot}79.\sin p$	$+0{\cdot}26.\sin(2p+109)$	$+0{\cdot}08.\sin(3p+166)$	$+0{\cdot}01.\sin(4p+233)$
	Small tides.	$4{\cdot}81.\sin p$	$+0{\cdot}13.\sin(2p+143)$	$+0{\cdot}07.\sin(3p+180)$	$+0{\cdot}02.\sin(4p+241)$
Kingstown	Large tides.	$5{\cdot}26.\sin p$	$+0{\cdot}38.\sin(2p+143)$	$+0{\cdot}02.\sin(3p+7)$	$+0{\cdot}02.\sin(4p+118)$
	Small tides.	$3{\cdot}69.\sin p$	$+0{\cdot}23.\sin(2p+165)$	$+0{\cdot}01.\sin(3p+225)$	$+0{\cdot}02.\sin(4p+171)$
Dunmore East	Large tides.	$5{\cdot}73.\sin p$	$+0{\cdot}14.\sin(2p+12)$	$+0{\cdot}15.\sin(3p+94)$	$+0{\cdot}01.\sin(4p+288)$
	Small tides.	$3{\cdot}91.\sin p$	$+0{\cdot}11.\sin(2p+3)$	$+0{\cdot}08.\sin(3p+103)$	$+0{\cdot}01.\sin(4p+198)$
New Ross	Large tides.	$6{\cdot}16.\sin p$	$+0{\cdot}47.\sin(2p+348)$	$+0{\cdot}26.\sin(3p+101)$	$+0{\cdot}03.\sin(4p+116)$
	Small tides.	$4{\cdot}51.\sin p$	$+0{\cdot}30.\sin(2p+346)$	$+0{\cdot}16.\sin(3p+95)$	$+0{\cdot}01.\sin(4p+63)$
Passage West	Large tides.	$5{\cdot}94.\sin p$	$+0{\cdot}29.\sin(2p+298)$	$+0{\cdot}09.\sin(3p+87)$	$+0{\cdot}04.\sin(4p+73)$
	Small tides.	$4{\cdot}19.\sin p$	$+0{\cdot}24.\sin(2p+316)$	$+0{\cdot}07.\sin(3p+95)$	$+0{\cdot}02.\sin(4p+43)$
Castle Townsend	Large tides.	$4{\cdot}79.\sin p$	$+0{\cdot}21.\sin(2p+297)$	$+0{\cdot}04.\sin(3p+259)$	$+0{\cdot}02.\sin(4p+331)$
	Small tides.	$3{\cdot}33.\sin p$	$+0{\cdot}16.\sin(2p+310)$	$+0{\cdot}03.\sin(3p+302)$	$+0{\cdot}03.\sin(4p+73)$

In order to institute a comparison of these numerical results with the formulæ given by theory, it may be convenient to premise the following expressions:—

If the tide were created in a uniform channel, forming a great circle round the earth, the expression for the height of the water would have the form

$$A.\sin p + B.\sin \overline{2p+90°},$$

where B would have the same sign as A if the velocity of the tide-wave were greater than $\sqrt{\frac{gk}{4}}$, k being the depth of the channel.

If the tide at the mouth of a gulf be a pure tide, or one in which the elevation is expressed by $a.\sin p$, then the elevation at any point in the bay will be expressed by the formula

$$A.\sin p + B.\sin \overline{2p+90°},$$

where (in the case of a gulf sufficiently long to have a tidal node) A and B have different signs from the mouth of the gulf to the node, and afterwards have similar signs.

If the tide at the mouth of an indefinitely long river be a pure tide, then the elevation at any point in the river will be expressed by the formula

$$A.\sin p + B.\sin 2p,$$

where B has the same sign as A if the section of the river be a parallelogram, but may have the opposite sign if the section expand very much at the top.

In all cases $\frac{B}{A}$ is a quantity of the same order as $\frac{\text{vertical oscillation of the water}}{\text{depth of the water}}$.

I shall now proceed to examine the results deducible from the last Table.

The stations Kilbaha, Casleh Bay, Old Head, Mullaghmore, may fairly be considered as littoral stations on the open Atlantic ocean. And their formulæ agree among themselves almost absolutely to the second term, and in a great measure to the third term. They show clearly that the Atlantic tide there is not a pure tide. But the form of the argument does not agree with either of the two first formulæ just cited, which alone can apply to it. At Castle Townsend, which is nearly as much exposed, but on a different side of the island, the formula agrees pretty well with the first of those above, supposing the depth of the sea very great.

At Dunmore East the tide has nearly assumed the form of a river tide.

Proceeding from Kingstown (where the character of the tide is similar to that at Kilbaha, &c.) to Clogher Head, Ardglass, and Donaghadee, the argument of the second term undergoes a progressive change, its phase being less advanced. As the epoch of the first term is absolutely the same at these stations (the tide being simultaneous at all of them), it appears that the wave represented by the second term is progressive. It seems therefore that it does not originate in the peculiarities of a gulf-tide (contained in the second formula just cited), but that it has been created either on the open sea or in the shallower water between Ireland and Cornwall, and now travels on as an independent wave.

From Mullaghmore to Port Rush, the second wave appears to travel with the same speed as the principal wave. In passing through the narrow channel to Glenarm, its phases appear to increase much more rapidly than those of the principal wave; or, it appears to travel more slowly, or in the opposite direction. This however is probably only an instance of the forced progression of the phases of wave in the channel connecting two tidal seas.

The general relations however of the waves depending on $2p$ at the different littoral stations will be seen more clearly from the following process:—Take the establishment of each station from Section XIII., and convert it into degrees at the rate of 1440° for a tidal day, and subtract the angle thus found from the angle added to $2p$. It is evident now that our phase p' at every station is referred to the same origin, namely to the time of the moon's transit at Greenwich. The expressions thus obtained for the quarto-diurnal waves are the following:—

Station.	Large tides.	Small tides.	Station.	Large tides.	Small tides.
	ft.	ft.		ft.	ft.
Kilbaha	$0{\cdot}16.\sin(2p'+244^\circ)$	$0{\cdot}08.\sin(2p'+295^\circ)$	Donaghadee ..	$0{\cdot}07.\sin(2p'+122^\circ)$	$0{\cdot}03.\sin(2p'+41^\circ)$
Casleh Bay..	$0{\cdot}16.\sin(2p'+235)$	$0{\cdot}07.\sin(2p'+296)$	Ardglass.....	$0{\cdot}18.\sin(2p'+163)$	$0{\cdot}07.\sin(2p'+199)$
Galway	$0{\cdot}18.\sin(2p'+223)$	$0{\cdot}08.\sin(2p'+277)$	Clogher Head..	$0{\cdot}26.\sin(2p'+167)$	$0{\cdot}13.\sin(2p'+218)$
Old Head ..	$0{\cdot}08.\sin(2p'+208)$	$0{\cdot}04.\sin(2p'+278)$	Kingstown	$0{\cdot}38.\sin(2p'+195)$	$0{\cdot}23.\sin(2p'+235)$
Mullaghmore	$0{\cdot}06.\sin(2p'+198)$	$0{\cdot}04.\sin(2p'+263)$	Dunmore East .	$0{\cdot}14.\sin(2p'+72)$	$0{\cdot}11.\sin(2p'+71)$
Port Rush ..	$0{\cdot}08.\sin(2p'+113)$	$0{\cdot}04.\sin(2p'+190)$	Passage West..	$0{\cdot}29.\sin(2p'+344)$	$0{\cdot}24.\sin(2p'+10)$
Ballycastle ..	$0{\cdot}07.\sin(2p'+203)$	$0{\cdot}06.\sin(2p'+245)$	Castle Townsend	$0{\cdot}21.\sin(2p'+7)$	$0{\cdot}16.\sin(2p'+27)$
Glenarm	$0{\cdot}17.\sin(2p'+156)$	$0{\cdot}05.\sin(2p'+147)$			

The expressions for Courtown, the station intermediate between Kingstown and Dunmore East, are (as I remark by anticipation from the next section) intermediate between those for Kingstown and Dunmore East, but nearly coinciding with the former.

The variations in the values of the constants attached to $2p$ in the Table of page 113, seem to make it impossible for us to attribute this term to the local circumstances of each port, and the consideration of the Courtown tides in the next section will confirm this. The order of the numbers attached to $2p'$ in the last Table shows that it may be considered as a progressive wave, beginning at Kingstown nearly, and travelling both ways round the coast as far as Donaghadee. But whether such a thing is mechanically possible, or whether it can be true that the quarto-diurnal wave (which necessarily is created by the semidiurnal wave flowing over the shallower seas between Ireland and Cornwall) can show itself as a great swell opposite Kingstown, and can then be propagated even opposite to the semidiurnal wave and round the island, are points which I cannot explain.

On the whole, I am not able to pronounce with any confidence on the origin of this wave, but I have no doubt that, having been created, it travels along independently, and therefore that its existence is not due to the local circumstances of the several stations.

In regard to the river stations, I may remark that the Shannon does, in consequence of the barriers to the tide at Limerick, resemble a gulf in its tidal character; and the second of the formulæ above ought therefore to apply to it; and, as will easily be seen, it does apply with considerable approximation. The river Barrow, upon which New Ross is situated, is not obstructed in the same manner, and therefore we might expect the third formula to apply, and it does apply very nearly.

I omit discussion of the third and fourth terms, because theory, in a shape applicable to cases of nature, has not yet been extended sufficiently far. I may however observe that, as I have shown with regard to the tide at Deptford*, and to those at Southampton and Ipswich†, so also at Limerick, and in some measure at Foynes Island and New Ross, the third term is almost as important as the second, and the fourth is one of considerable magnitude.

I may also call the attention of the wave-theorist to this circumstance, that the difference between the coefficients for large tides and for small tides does not appear sufficiently great in relation to the difference between the coefficients of the first term for large tides and small tides. The coefficient of the first term being considered as of the first order, that of the second term would consist of a series whose leading term was of the second order, &c. The departure from the proportions given by this consideration may depend upon the succeeding terms of the series.

It is also to be remarked that there is an undoubted difference between the arguments for large tides and for small tides. This seems to show that each sine is accompanied by a cosine, and that their coefficients have, for their leading terms, terms of different orders in respect of the first coefficient of semi-range.

Section XVII.—*Separate discussion of the tidal observations made at Courtown.*

The observers at Courtown soon discovered that it was impossible to adhere to the instructions sketched in Section I. The tide was sometimes apparent as a semidiurnal tide, but with considerable irregularity; at other times, the character of semidiurnal tide was (to common observation) completely lost, and in its stead there was a small tide four times a day; in all cases the tide was small. In this state of things, the course which they adopted was, to observe continuously whenever the semidiurnal tide was not distinctly marked, and to follow the usual rule (with some extension of observations) when it was well-marked. In this manner a tolerably complete and very important set of observations has been secured. In several cases the observations have been interrupted by the discharge of water from the sluices for scouring the harbour of Courtown; in some instances there has been no difficulty in filling up the observations by conjecture, in others I have been obliged to adopt the limits of the tide (in the form of analysis) to these interruptions.

In order to apply the method of analysis explained in Section X., it was necessary to fix upon precise limits for each tide. But as no limits could be obtained at Cour-

* Philosophical Transactions, 1842, p. 4. † Ibid. 1843, pp. 49 and 53.

town (as at other stations) from the observations themselves, it was necessary to take them from another station. The station selected for this purpose was Ardglass. The limits and the divisions into twelfths and sixteenths for Ardglass were therefore adopted for Courtown, as far as the continuity of observations permitted. Where (as is just mentioned) it was necessary to change the limits, this was done if possible by altering the limits and all the divisions by three or six of the twelfths (corresponding to four or eight of the sixteenths); in other cases they were all altered by a definite time. Then the means of the heights in these observations were treated in the usual way.

A correction for diurnal tide was indispensable (the diurnal tide being, at some times, as large as the semidiurnal). For obtaining this from the observations there were two means. One was, by means of the investigations connected with tertio-diurnal tide to be detailed in the next section. These gave the diurnal tide for the beginning and the fourth parts of each of the whole day's group used there. These were the diurnal coefficients proper to be used in the semidiurnal groups composing each day's group. But the corresponding diurnal coefficient applicable at the times of any Ardglass high or low water was easily deduced from them by taking the sum of the products of the coefficients next it by the cosines of their respective distances from it (considering 360° as corresponding to a tidal day). Another method was, to select from the observed heights those which corresponded to the times of Ardglass high water and Ardglass low water, and to treat them by the method of fourth differences explained in Section III.; as these heights ought (in relation to each other) to be perfectly free from the effect of semidiurnal tide and of all tides occurring at portions of a semidiurnal tide. Using then these two methods, and adopting the mean of their results when both could be applied, a number of diurnal coefficients were obtained from the observations themselves. On comparing these with the diurnal coefficients at the neighbouring stations, it was found that the coefficients at Courtown might very well be represented by the mean of those for Kingstown and Dunmore East at the same time. Accordingly, for all the times for which no diurnal tide could be safely extracted from the observations, the mean of coefficients for Kingstown and Dunmore East was used; and from these, when necessary, the coefficients for other times were deduced by the operation described above. The process then pursued was exactly the same as in other cases, except that no correction was attempted for rise of water. The results are the following, which differ in form from preceding results only in this circumstance, that the origin of phase is the time of mean water at Ardglass preceding high water, and that therefore an angle expressed by a number of degrees must be added to the phase to form the argument of the first variable term.

Height of the water in each individual tide at Courtown, excluding diurnal tide, where the origin of p is at the same time as at Ardglass (p. 82 and 83),

Analysed time of high water, corresponding to $p+c_1=90$.	A_0.	C_1.	c_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	°	ft.	°	ft.	°	ft.	°
June 22. 20 13	16·93	0·74	109	0·43	170	0·03	320	0·02	103
23. 8 30									
23. 20 46	17·30	0·74	91	0·36	133	0·05	93	0·02	265
24. 9 25									
24. 22 4	17·55	0·50	73	0·36	102	0·04	180	0·04	20
25. 10 11									
25. 22 18	17·03	0·78	83	0·33	127	0·08	253	0·05	337
26. 10 13									
26. 22 9 / 27. 10 21	16·55	0·46	107	0·33	122	0·03	178	0·04	200
27. 22 5 / 28. 10 30	16·58	0·38	125	0·29	128	0·04	160	0·04	152
28. 22 39	16·66	0·17	133	0·22	130	0·03	238	0·03	298
29. 11 24	16·64	0·25	112	0·05	130	0·05	106	0·01	217
29. 22 42	16·71	0·17	199	0·24	130	0·03	275	0·02	314
30. 9 29	16·59	0·18	199	0·28	148	0·03	164	0·03	60
30. 21 3	16·69	0·36	251	0·19	126	0·02	59	0·03	237
July 1. 8 50	16·65	0·23	239	0·19	138	0·04	164	0·04	227
1. 21 14	16·66	0·28	256	0·23	128	0·03	280	0·03	333
2. 9 21									
2. 16 7 / 3. 4 42	16·88	0·09	58	0·18	83	0·03	296	0·05	137
3. 17 26 / 4. 5 51	17·59	0·13	51	0·15	106	0·04	127	0·05	200
4. 22 34 / 5. 11 4	17·40	0·12	291	0·21	143	0·12	114	0·04	358
5. 18 38 / 6. 7 8	16·32	0·51	74	0·29	134	0·05	50	0·04	350
6. 19 52 / 7. 8 12	17·21	0·37	63	0·52	120	0·11	207	0·05	43
.....	....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....	....
8. 20 42 / 9. 9 12	16·65	1·29	81	0·44	132	0·10	138	0·05	260
9. 21 32	16·62	1·57	82	0·37	134	0·11	87	0·07	231
10. 9 58									
10. 22 25	17·62	1·18	77	0·16	96	0·09	101	0·08	66
11. 10 40									
11. 22 56	16·48	1·26	82	0·34	116	0·10	100	0·06	202
12. 11 24									
12. 23 51	16·59	0·93	86	0·42	127	0·01	1	0·03	199
(13. 9 59)	....	....	....	....	....	....	....	....	....
(13. 20 7)	....	....	....	....	....	....	....	....	....
(14. 6 15)	....	....	....	....	....	....	....	....	....
(14. 16 23)	....	....	....	....	....	....	....	....	....
15. 2 31	16·08	0·27	58	0·43	121	0·04	282	0·07	345
15. 17 56 / 16. 6 26	16·58	0·04	339	0·24	136	0·06	329	0·02	192
16. 20 45	17·05	0·26	288	0·33	106	0·08	101	0·11	178
17. 9 36	17·11	0·09	278	0·15	150	0·11	328	0·10	358
17. 22 4	16·89	0·10	283	0·38	135	0·01	160	0·03	164
18. 2 39	16·90	0·01	155	0·35	115	0·17	106	0·08	231
18. 15 45	16·81	0·18	130	0·37	142	0·10	40	0·12	113
19. 5 18	16·82	0·17	93	0·29	115	0·16	304	0·06	304
19. 19 5	16·70	0·21	53	0·35	125	0·14	122	0·10	131
20. 7 58	16·80	0·58	68	0·22	151	0·04	294	0·03	341
20. 19 41	16·61	0·57	93	0·36	138	0·05	169	0·05	233
21. 7 35									
21. 20 2 / 22. 8 22	16·47	0·74	91	0·28	141	0·04	131	0·04	12

expressed by the formula $A_0+C_1\sin(p+c_1)+C_2\sin(2p+c_2)+C_3\sin(3p+c_3)+C_4\sin(4p+c_4)$, and p increases by 360° during one complete tide at Ardglass.

, Analysed time of high water, corresponding to $p+c_1=90°$.	A_0.	C_1.	c_1.	C_2.	c_2.	C_3.	c_3.	C_4.	c_4.
1842. h m	ft.	ft.	°	ft.	°	ft.	°	ft.	°
July 22. 20 40	16·32	0·80	89	0·34	135	0·05	152	0·01	158
23. 8 54									
23. 21 9	16·32	0·89	96	0·37	145	0·06	79	0·07	158
24. 9 18									
24. 21 28	16·51	0·74	103	0·23	125	0·12	211	0·05	198
25. 9 41									
25. 21 54	16·37	0·73	105	0·35	128	0·07	248	0·02	270
26. 10 17	16·36	0·80	102	0·21	155	0·12	106	0·02	75
26. 22 25	16·41	0·60	111	0·27	124	0·06	168	....	....
27. 10 35	16·34	0·61	106	0·25	129	0·04	130	....	....
27. 23 13	16·42	0·39	108	0·30	117	0·04	155	0·03	341
28. 11 7	16·37	0·35	109	0·10	127	0·06	171	0·04	161
28. 22 37	16·44	0·32	158	0·23	128	0·07	148	0·01	222
29. 10 2	16·36	0·24	163	0·22	132	0·04	254	0·05	341
29. 21 25	16·38	0·25	208	0·24	109	0·02	61	0·03	43
30. 8 42	16·31	0·25	233	0·24	134	0·05	10	0·05	0
30. 20 35	16·25	0·41	247	0·18	116	0·01	105	0·03	245
31. 8 38	16·21	0·43	259	0·19	150	0·01	249	0·02	18
31. 20 55									
August 1. 9 8	16·37	0·48	266	0·27	118	0·02	87	0·02	107
1. 21 6	16·53	0·34	287	0·21	148	0·02	46	0·02	36
2. 9 20	16·57	0·36	305	0·25	149	0·01	226	0·02	78
2. 19 45	16·42	0·09	9	0·26	150	0·05	30	0·03	122
3. 7 36	16·32	0·22	31	0·31	159	0·02	63	0·02	39
3. 18 27	16·48	0·35	83	0·20	148	0·06	110	0·03	177
4. 6 44	16·47	0·60	82	0·26	170	0·09	45	0·09	131
4. 19 17	16·76	0·88	79	0·35	152	0·04	214	0·07	27
5. 8 1	16·73	1·13	69	0·41	156	0·04	216	0·05	80
5. 20 0	16·58	1·23	80	0·33	140	0·09	53	0·08	55
6. 8 18									
6. 20 36	16·65	1·53	78	0·41	110	0·11	128	0·05	3
7. 9 5									
7. 21 34	16·46	1·89	75	0·45	141	0·03	169	0·06	127
8. 9 56									
8. 22 18	16·64	1·70	74	0·52	127	0·07	118	0·01	225
9. 10 4									
9. 21 50	17·31	1·60	114	0·28	225	0·21	48	0·09	226
10. 10 45									
10. 23 40	16·56	1·32	82	0·33	127	0·11	269	0·09	215
11. 11 57									
12. 0 14	16·62	0·59	88	0·41	142	0·02	329	0·03	141
12. 13 15	16·63	0·58	72	0·22	120	0·05	222	0·03	59
12. 23 49	16·26	0·09	125	0·29	120	0·06	356	....	....
13. 9 26	16·29	0·11	209	0·31	123	0·11	29	0·01	247
13. 20 54	16·20	0·25	274	0·27	117	0·07	88	0·11	187
14. 8 41	16·29	0·34	260	0·39	150	0·05	354	0·01	218
14. 21 8	16·32	0·34	287	0·35	126	0·06	243	0·03	235
15. 9 33	16·35	0·39	277	0·21	130	0·04	27	0·02	208
15. 21 50	16·55	0·13	307	0·17	148	0·03	157	0·02	350
16. 10 6	16·31	0·25	300	0·24	113	0·08	94	0·05	178
16. 22 50									
17. 6 30	16·78	0·21	78	0·14	122	0·05	169	0·02	47
17. 18 41	16·58	0·19	84	0·28	120	0·07	250	0·08	325
18. 7 24									
18. 20 7	17·07	0·17	64	0·30	122	0·05	76	0·04	90
.....	....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....	....
.....	....	....	....	....	....	....	....	....	....
20. 20 20	16·57	0·92	89	0·23	123	0·04	124	0·02	267

Upon inspecting the numbers in the column headed c_1, it will be perfectly evident that there is an error on August 9. The semidiurnal tide on that day is (comparatively) large, its whole range exceeding three feet; and there is no instance throughout all the observations of an irregularity equal to that which corresponds to an anomaly of 30° with that range. I have no doubt that all the observations are recorded too early by one hour, an error which would easily be committed at beginning, and which, where the observations are entered on forms ready prepared, would be retained to the end. Correcting on this supposition, the three successive times of high water would be August $9^d\ 10^h\ 34^m$, August $9^d\ 22^h\ 50^m$, and August $10^d\ 11^h\ 15^m$; and the values of c_1, c_2, c_3, c_4, for August $9^d\ 22^h\ 50^m$ would be nearly 84°, 165°, 218°, and 106°.

Next I would remark that there is undoubtedly an error of the same kind in the tide of July 4–5; but as the tide is then very small, I have not ventured to state precisely the alteration which I would propose.

Thirdly, observing that where the tide is very small, the hours on successive days occur earlier, in each of the instances where the order is well-marked (as from June 27 to July 3, from July 27 to July 31, and from August 12 to August 14), there can be no doubt that the same thing must hold during the interruption of observations about July 13 and 14; and thus it will be seen that there are certainly four high waters lost at that time. I have inserted four numbers by simple interpolation, to show, within two or three hours, the times of the lost high waters.

It is also to be remembered that nothing can be inferred from such tides as those of July 15–16 and July 18, where the coefficients are 0·04 and 0·01.

Bearing these remarks in mind, and giving particular attention to the second half of the observations, which, both for the regularity of the system pursued by the observers and for the agreement of the results, is greatly superior to the first, we arrive at the following conclusions:—

The angle c_1 increases continually, and its increase amounts to 360° in about fourteen days. When its value is not far from 360°, its increase is extremely rapid. One of these jumps occurs between August $2^d\ 9^h$ and August $2^d\ 19^h$, and one between August $16^d\ 22^h$ and August $17^d\ 6^h$; one also between July $2^d\ 9^h$ and July $2^d\ 16^h$; another takes place between July $17^d\ 22^h$ and July $19^d\ 5^h$, but (the results being at that time somewhat irregular) the time cannot be precisely pointed out. It is evident from this that the Courtown tides are more numerous than the Ardglass tides by one tide in fourteen days nearly.

The mean solar time of high water does not increase constantly as at other stations, but oscillates backwards and forwards. Thus, from July 19 to August 18 (in which period the tides at other stations have gradually retarded by twenty-four hours), the evening tides always occur between $5^h\ 18^m$ and $13^h\ 15^m$, and the morning tides always occur between $18^h\ 27^m$ and $24^h\ 14^m$, each time having twice oscillated between its extreme limits in that period.

From these circumstances it is plain that the time of high water (confining our remarks to the term $C_1 \sin(p+c_1)$) respects mainly the time of the sun's transit and not that of the moon's transit; and therefore, at Courtown, *the solar tide is greater than the lunar tide.* This is, I believe, the only place on the earth in which such a result has been distinctly obtained. The observations of Sir EDWARD BELCHER* show that at Otaheite the solar tide is as nearly as possible equal to the lunar tide.

The times following lunar syzygy by two days were June 24, 9^h, July 9, 19^h, July 23, 23^h, August 8, 4^h, August 22, 16^h; and about these times the luni-solar tide is greatest at all the other stations. About these times also the soli-lunar tide at Courtown is greatest.

The solar hour of high water at Kingstown, at the highest tides, is about $12^h\ 30^m$, that at Dunmore East is about $6^h\ 40^m$. These are the two stations nearest to Courtown on the north and south sides. The solar hour of high water at Courtown at the same times is about $9^h\ 30^m$. At these times the effects of the sun and the moon are simultaneous as to phase, so that we may treat the result as if there were only a single wave. It would seem then that the transition from a tide of elevation at Dunmore East to one of simultaneous depression at Kingstown, and *vice versâ*, is not effected entirely by a node dividing the elevated wave from the depressed wave. It appears that there is also a small progressive wave. The geometrical representation appears to be this; that there is a large stationary wave, having a node near Courtown, and making high water simultaneous in all parts of the inland sea or Irish Sea, and synchronic with low water in the exterior sea; and that there is mingled with it a very small progressive wave. As to the mechanical explanation of it, I can offer nothing positive. But I would suggest for the consideration of wave-theorists, whether, in the case of a gulf (as the Irish Sea) having a small outlet (like the North Channel), it be possible that the fluctuation may be represented correctly on mechanical principles by a combination of the stationary wave peculiar to a gulf with the progressive wave peculiar to the channel.

Returning now to the consideration of the magnitude of the tide, it is evident that the coefficient of the lunar tide has been diminished in a far greater degree than that of the solar tide. There is one explanation of this which is very plausible, and which I have no doubt is the true one, namely that the node for the lunar tide and the node for the solar tide do not coincide (which, on account of the difference of the periods of these tides, we should expect *à priori*), and that the node for the lunar tide is much nearer to Courtown than is the node for the solar tide. It is clearly possible that, by varying our choice of stations, we might vary the proportion of the two effects in any degree whatever. Nay, by choosing a station between the two nodes, we might have the solar and lunar effects to conspire when they are opposed at other places, and *vice versâ*; and thus a station would be found where the spring tides occur at the same time as neap tides at other places. This does not occur at

* Philosophical Transactions, 1843, p. 55, &c.

Courtown; but the reader, in reflecting on this, will see the importance of our comparison of the time of the largest tides at Courtown with the time of the largest tides at the other stations.

We shall now proceed to examine the second periodical term; which will be found not less remarkable than the first.

On glancing over the values of c_2 (first correcting that on August 9 as I have suggested), the reader cannot fail to be struck with the general uniformity of the numbers. It is quite evident that this term has no respect to the sun's transit, but that it respects only the moon's transit, or the commencement of the luni-solar tide. If we look also to the coefficient C_1, we find that its magnitude is considerable, sometimes exceeding that of the first term. It is clear therefore that this term does not originate as a derivative from the first term, produced by the local circumstances of the port. It does not change greatly, but nevertheless has on the whole larger values about the times of large tides than about the times of small tides. If we divide the observations into two groups, one corresponding to large tides and the other to small tides (the limits being the same as those for the other stations); and if we correct as before for the establishment at Ardglass (to which station the Courtown tides have been referred); and if we collect the expressions for the second periodic term at the three stations, Dunmore East, Courtown and Kingstown; we have this sequence of expressions.

	Kingstown.	Courtown.	Dunmore East.
Large tides	ft. $0{\cdot}38 \cdot \sin(2p' + 195^\circ)$	ft. $0{\cdot}35 \cdot \sin(2p' + 196^\circ)$	ft. $0{\cdot}14 \cdot \sin(2p' + 72^\circ)$
Small tides.....	$0{\cdot}23 \cdot \sin(2p' + 235)$	$0{\cdot}26 \cdot \sin(2p' + 202)$	$0{\cdot}11 \cdot \sin(2p' + 71)$

It appears here quite evident that this term at Courtown is only the representation of the same quarto-diurnal tide which shows itself along the whole coast. This wave (whatever its origin may be) appears to have its greatest range and its beginning of phases at Kingstown, and to spread both ways, diminishing in range as it goes.

The succeeding terms at Courtown are insignificant.

We have now a clear representation of the apparently confused phenomena of the tides at Courtown. Both the semidiurnal tides are very much diminished, the lunar so much that its range is rather less than that of the solar tide. The quarto-diurnal tide exists in nearly its greatest magnitude. The geometrical representation is perfect; the mechanical explanation is not complete. In both respects, as regards what is reduced to law and what is yet incomplete, the Courtown tides must be regarded as the most remarkable that have ever been examined.

Section XVIII.—*Examination into the question of tertio-diurnal tide.*

The observations at Courtown, as has been mentioned, and as appears from the Table in pages 118 and 119, were continued without interruption, day and night, for

a considerable time. The observations at Dunmore East, as appears from the beginning of the Table in page 88, were also continued without interruption for several days. These circumstances appeared to me to offer a convenient opportunity of examining whether the tide occurring three times in the lunar day, which is pointed out by theory, is sensible in the seas around Ireland.

The calculations for this purpose were made in the same manner as the other calculations described in Section X. The means for the twelfth parts of semidiurnal tide, or for the twenty-fourth parts of diurnal tide, having been already found for the operations in Section X., the means of the first and second, of the third and fourth, of the fifth and sixth, &c. were taken; and these were evidently the same as the means for the twelfth parts of diurnal tide. They were then treated by the use of the printed skeleton form shown in Section X., in the same manner as the means for the twelfth parts of semidiurnal tide. Thus the diurnal tide and tertio-diurnal tide were obtained; and a consideration of the principles on which that process is founded will show that the result is in no way affected by the semidiurnal or quarto-diurnal tide. The constants additive to the phase, at Courtown, were corrected where necessary to adapt them to the supposition that the phases are measured from the mean water preceding High Water, First Division, at Ardglass; those at Dunmore East are referred to the same state of tide at Dunmore East. The angle p in the following Table increases by 360° in a tidal day. The day which is set down is that whose astronomical commencement occurs in the tidal day (the limits of the tidal day will be seen in Section X.).

Courtown.			Dunmore East.		
Day.	Diurnal tide.	Tertio-diurnal tide.	Day.	Diurnal tide.	Tertio-diurnal tide.
	ft.	ft.		ft.	ft.
June 29.	$0{\cdot}57 \cdot \sin(p+250^\circ)$	$0{\cdot}08 \cdot \sin(3p+188^\circ)$	June 24.	$0{\cdot}33 \cdot \sin(p+208^\circ)$	$0{\cdot}16 \cdot \sin(3p+253^\circ)$
30.	$0{\cdot}31 \cdot \sin(p+253)$	$0{\cdot}04 \cdot \sin(3p+86)$	25.	$0{\cdot}35 \cdot \sin(p+207)$	$0{\cdot}08 \cdot \sin(3p+304)$
July 1.	$0{\cdot}43 \cdot \sin(p+264)$	$0{\cdot}02 \cdot \sin(3p+177)$	26.	$0{\cdot}20 \cdot \sin(p+192)$	$0{\cdot}11 \cdot \sin(3p+220)$
17.	$0{\cdot}20 \cdot \sin(p+350)$	$0{\cdot}14 \cdot \sin(3p+207)$	27.	$0{\cdot}27 \cdot \sin(p+170)$	$0{\cdot}10 \cdot \sin(3p+210)$
18.	$0{\cdot}05 \cdot \sin(p+201)$	$0{\cdot}06 \cdot \sin(3p+236)$	28.	$0{\cdot}33 \cdot \sin(p+161)$	$0{\cdot}09 \cdot \sin(3p+182)$
19.	$0{\cdot}24 \cdot \sin(p+265)$	$0{\cdot}05 \cdot \sin(3p+279)$	29.	$0{\cdot}32 \cdot \sin(p+150)$	0·00
20.	$0{\cdot}36 \cdot \sin(p+261)$	$0{\cdot}23 \cdot \sin(3p+187)$	July 1.	$0{\cdot}19 \cdot \sin(p+187)$	$0{\cdot}07 \cdot \sin(3p+349)$
26.	$0{\cdot}38 \cdot \sin(p+256)$	$0{\cdot}06 \cdot \sin(3p+179)$	2.	$0{\cdot}17 \cdot \sin(p+178)$	$0{\cdot}03 \cdot \sin(3p+99)$
27.	$0{\cdot}43 \cdot \sin(p+253)$	$0{\cdot}05 \cdot \sin(3p+132)$			
28.	$0{\cdot}49 \cdot \sin(p+240)$	$0{\cdot}06 \cdot \sin(3p+158)$			
29.	$0{\cdot}29 \cdot \sin(p+248)$	$0{\cdot}08 \cdot \sin(3p+84)$			
30.	$0{\cdot}25 \cdot \sin(p+276)$	$0{\cdot}08 \cdot \sin(3p+87)$			
31.	$0{\cdot}15 \cdot \sin(p+287)$	$0{\cdot}10 \cdot \sin(3p+98)$			
Aug. 2.	$0{\cdot}16 \cdot \sin(p+10)$	$0{\cdot}01 \cdot \sin(3p+306)$			
3.	$0{\cdot}10 \cdot \sin(p+105)$	$0{\cdot}08 \cdot \sin(3p+315)$			
4.	$0{\cdot}38 \cdot \sin(p+106)$	$0{\cdot}02 \cdot \sin(3p+3)$			
5.	$0{\cdot}53 \cdot \sin(p+87)$	$0{\cdot}11 \cdot \sin(3p+328)$			
12.	$0{\cdot}34 \cdot \sin(p+57)$	$0{\cdot}12 \cdot \sin(3p+346)$			
13.	$0{\cdot}11 \cdot \sin(p+357)$	$0{\cdot}07 \cdot \sin(3p+238)$			
14.	$0{\cdot}09 \cdot \sin(p+46)$	$0{\cdot}11 \cdot \sin(3p+211)$			
15.	$0{\cdot}05 \cdot \sin(p+286)$	$0{\cdot}07 \cdot \sin(3p+264)$			
16.	$0{\cdot}26 \cdot \sin(p+286)$	$0{\cdot}04 \cdot \sin(3p+70)$			

The diurnal tide is here shown pretty well, and the times of its changes of sign agree well with those found in Section III. But the numbers for tertio-diurnal tide appear to me perfectly lawless. I think that they must be regarded as principally the effects of accident.

On the whole, I am inclined to believe that, as far as evidence goes, the tertio-diurnal tide is not sensible on the coast of Ireland.

II. *On the Temperature of the Springs, Wells and Rivers of India and Egypt, and of the Sea and Table-lands within the Tropics. By Captain* NEWBOLD, *Madras Army, F.R.S.*

Received January 1, 1842,—Read February 22, 1844*.

PROFESSOR JAMESON, in his chapter on the hydrography of India, justly remarks, "Although India, like other great tracts of country, contains many springs, these have hitherto attracted but little attention. The temperature of but few of them is known; their magnitudes and geognostical situations are scarcely ever mentioned; and their chemical composition, excepting in a very few instances, has been neglected. The most important feature in the natural history of *common* or *perennial* springs, namely their temperature, is rarely noticed, although a knowledge of this fact is illustrative, not only of the mean temperature of the climate, but also of the elevations of the land above the level of the sea; and our information in regard to their chemical nature is equally meagre†." Since the publication of these remarks, much has been done by PRINSEP and others in these branches of Indian hydrography, but more remains to be effected before this reproach can be wiped out. The heat of springs having a temperature little above the mean of that of the surrounding country has been rarely noticed, though I feel convinced many such exist in India. That of springs of high temperature, more attractive to the casual observer, has been more remarked.

My own observations, and the few inferences I have ventured to draw from some of them, are not offered as sufficient data for the establishment of laws, but merely as a contributory mite to knowledge; in the view of courting inquiry and observation by others more competent and better situated for continued research than myself. The thermometric observations have been snatched generally on the line of march, or during hasty travel: since my return to England, through the kindness of Mr. ROBERTON, they have been adjusted to the indications given by the standard thermometer of the Royal Society.

The observations extend at irregular intervals from Alexandria to Malacca, or from 31° 13′ of north latitude to within 2° 14′ north from the equator; and between the meridians of 27° and 103° of east longitude. I had continued those on the temperature of the sea as far as the Bosphorus and Black Sea, but have judged them

* This paper having unfortunately been mislaid after its receipt by the late Assistant Secretary, the reading of it was thus necessarily delayed.—S.H.C.

† Ed. Cab. Cyc. No. 8. p. 287.

superfluous in a paper limited almost to the subject of intertropical temperature.

In the columns of the registers, the latitude and longitude, the approximate height above the sea, the nature of the surrounding formation, the depth to the surface of the water and depth of water, the temperature of the air, the month during which each observation was taken, and the approximate annual mean of the climate in which the wells, &c. occur, are specified as far as practicable. In the column of remarks will be found a few observations on the chemical nature of the water, and on the size of the wells and springs*. Those were selected which contained water all the year round; though all were, more or less, subject to fluctuation during the wet and dry seasons. The wells in Egypt differ from the "*bouries*" of India in being less open and exposed to atmospheric influence. Those in the valley of the Nile are mere shafts sunk through the black alluvium to an impervious marly and sandy bed, to depths varying, according to the distance from the river, from ten to forty feet. Their circumferences, like those of the Indian "pot wells," are from nine to twelve yards. They mainly depend on the river for water, which is supplied by infiltration through the soil,—a circumstance to be taken into consideration in all indications afforded of their temperature. The wells in the deserts of Egypt, like those of Ajmír and the western deserts of India, are frequently of great depth, lying under strata of sand, gravel, and a calcareous sandstone, on an argillaceous or marly bed, sometimes at a depth of 300 feet below the surface of the surrounding country. In the granitic districts of Upper Egypt, in the Thebaid desert, however, I have observed springs rising through the almost vertical strata to the surface.

In India, most of the wells marked as occurring in granite, trap, limestone and sandstone, result from springs, and are consequently not so much influenced in temperature by the monsoon rains as those in lateritic rocks, which, from their porous structure, admit of the percolation of rain water to a considerable extent.

The temperature was generally taken at about 10 A.M., a time when I found it to approximate nearest the diurnal mean; and, whenever practicable, at the depth of about ten feet from the surface.

The following are the general results of many hundred observations:—

1st. In low latitudes the temperature of the deepest wells and springs is a little higher than the mean temperature of the air. Exceptions occur: for example, the temperature of a deep well at Gádigánúr, on the banks of the Toombuddra, between the 15th and 16th parallels of north latitude, at an elevation of about 1200 feet from the sea, was so low as 72°·5 (the temperature of the air in the shade, at the time of observation, 80°·5), while that of the springs and river in the vicinity was from 77° to 79°·5. Ranges of hills, attaining an altitude of 1500 feet above the plain, rose at no great distance; a circumstance suggesting the probability that the cold spring had its

* The observations of others will be denoted in the column of remarks by the names of the observers. The scale throughout is that of FAHRENHEIT.

source at an elevation having a mean temperature lower than that of the plain where the water appears on the surface.

2nd. The temperature of strongly saline and sulphureous springs is, on the average, higher than those of pure water.

3rd. Both saline and cold springs are seen to occur within a few feet from thermal and freshwater springs; a fact to be ascribed probably to their rising through different seams of the subjacent strata (often highly inclined), and to the different depths and heights from which the supply of water is derived.

4th. The temperature of wells, particularly those with a small area, much used for purposes of irrigation, is thereby artificially increased.

5th. The temperature of shallow exposed wells, springs and rivers, especially such as have sandy beds, is subject to great diurnal fluctuation, conforming, though to a less extent, to that of the superincumbent atmosphere. The surface water of deep wells partakes of this fluctuation, to a depth varying according to the transparency of the water, extent of surface, degree of exposure, and clearness of the sky. In muddy water the surface is heated to a greater extent, but a foot or two deep is less affected by the calorific action of the solar rays than clear water.

The transparent water of a large well at Bellary, lat. 15° 5′ N. and long. 76° 59′ E., situate on a table-land elevated 1600 feet above the sea's level, and containing sixteen feet of water, I found, at the depth of nine feet from the surface, to vary but one degree during the day, from sunrise to sunset, and this in several hundred experiments. The minimum, 79°·5, took place a little after sunrise, and the maximum, 80°·5, at 3 P.M. following those of the air. The diurnal variation of the water an inch below the surface amounted to 12°. During the commencement of the dry weather, as the heat increased, the water gradually decreased, and the diurnal fluctuations became greater, and increased at a greater rate than that of the decrease of the water.

Thermal Springs.—The thermal springs, both of India, the peninsula of Sinai and Egypt, are, with few exceptions, either mineral or gaseous. Those near the shores of the Red Sea are sulphureous; and strictly speaking, perhaps, should not be classed as thermal springs, from the great probability of their being connected with the volcanic belt that passes under the bed of the Red Sea, and bursting up from its watery fetters appears in the semi-dormant volcano of Gebel Teer, and in the lavas of Aden, beyond the straits of Babel-Mandel. The highest known temperature of the thermal springs is 102°, viz. that of El Kasr in the Oasis of Dakhleh; in the peninsula of Sinai, 91°6, that of the Hummám Músa, hot-baths of Moses (Wells of Elim?) near Tor. It is probable, from reports given me by the Arabs, that the Hummám Pharáon, hot-baths of Pharaoh, about eighty-five miles northerly from Tor, are of higher temperature. The maximum attained by the thermal springs of India is 194° at Jumnotri in North Hindostan (lat. 30° 52′ N.); a temperature almost equivalent, at that elevation—10849 feet above the sea's level—to the boiling point of water, and 18° higher than that of the hottest known thermal spring of Europe unconnected with present active vol-

canos, namely, 176° Fahr., that of Chaudes Aigues in Auvergne. The temperature of the hottest known thermal spring in the world, according to M. Arago, is that of Las Trincheras in Venezuela, stated, on the authority of Humboldt and Boussingault, to have increased 11° since 1806 to February 1823, viz. from 195° to 206° Fahr. Had M. Arago stated its elevation above the sea, a better comparison between its temperature and that of Jumnotri might have been formed. It would be interesting to observe whether any similar increment of heat takes place in the chain of thermal springs that rise abundantly along the great line of dislocation at the southern base of the Himálaya chain, or whether the temperature falls, as in some thermal springs among the East Pyrenees. It is certain that the majority of the springs strictly termed thermal, occur in India at or near lines of great faults occasioned by the upheaving of plutonic rocks, a fact that speaks intelligibly as to the great depth at which the earth's crust has been broken up.

Hot springs were found by Burnes in the salt districts of the Punjaub. In Thibet, M. Csoma de Koros mentions the occurrence of hot springs between U and Ts'ang. They are numerous in the mountains lying east from the Ma-p'-ham lake, especially at one place, where there is a hole out of which vapour continually issues, and at certain intervals, as in Iceland, hot water is ejected with great noise to the height of twelve feet. The water of the hot springs of Assam was found by Mr. J. Prinsep to contain bitumen and sulphuretted hydrogen. One held in solution a portion of muriate of soda. Many other warm springs are known to occur, besides those mentioned in the register, regarding the temperature and chemical composition of which further information is desirable. For instance, those of Hummám Pharáon on the east shore of the Red Sea; of Vizrabhaee, forty-eight miles north of Bombay; at Mohr on the Bancoot river, about seventy-five miles south of Bombay; of Soonup Deo, and Oonup Deo among the Satpoora hills in Khandésh; of Rishï-kúnda in Rajmahal; of Muktinath and Bhadrinath in North Hindustan; of Tooee, near Ruttenpore on the Mhye river, in Guzerat; of Lawsoondra, eighteen miles W.N.W. from Tooee; of Uteer, about thirty miles from Poorea near Korachi, on the Indus; of the diamond district at Punnah, in Bundelcund; of Oetha-gur, and Bannassa, near the sources of the Jumna; of the rivulet of Loland Khad near the Sutledge; of those near the confluence of the Soar and Elgie rivers with the Ganges; of many known to exist in the Birman empire and Malayan peninsula, and of Bhotan. The last-mentioned springs throw up spheroids of silex, which are brought to Almorah and there sold by the native merchants for duck shot*. These spheroids resemble those of the springs of Carlsbad in Bohemia, and of the Geysers. The silex composing them has doubtless been held in solution by the water; but it remains yet to be shown whether it contains, or not, that peculiar combination of silica and soda, which, according to Mr. Faraday, characterizes the water of the Geysers†; a combination ceasing to exist when the water is evaporated: the silica being then de-

* M'Lelland.

† Barrow's Visit to Iceland, pp. 209, 211.

posited in an insoluble condition, while the alkali, probably by the agency of the carbonic acid of the atmosphere, is set free, and remains dissolved in the water in considerable quantity. In Southern India many thermal springs, hitherto entirely unnoticed, are suspected to occur; Colonel SYKES states that he has been informed of their existence in Canara: I have heard of one among the Raidrúg hills in the Ceded districts,—in the Koondahs on the west coast,—and discovered another at the base of the hills south of Cuddapah having a temperature of 88°, as noted in the register. A spring near Salem in South India is probably thermal, having a temperature of 84°, ascertained for me by Mr. G. FISCHER.

Temperature of Rivers.—The supposition that the temperature of rivers is lower, from the influence of evaporation, radiation, and the elevation at which they rise, than that of the country through which they flow, appears subject to some modification as regards great streams whose course lies chiefly through equinoctial regions. Many, like the Nile, derive the great bulk of their water from the rains that fall periodically near the equator when the sun is nearly vertical, and evaporation reduced to its minimum from the saturated state of the atmosphere. The fallen waters derive additional heat in overspreading the wide extent of sand and alluvium that form and skirt the channels through which they roll on towards the ocean; and which, during great part of the year, have been left dry and freely exposed to the rays of a scorching sun. The beds of the most considerable rivers of South India present in many parts of their course, during the dry season, dreary wastes of arid sands, through which the river, reduced to a slender thread, barely finds its way to the sea. The mean of more than 200 observations, taken day and night, on the temperature of the Nile, in July, between Cairo and Thebes, I found to exceed the mean annual temperature of the air at Cairo (72°·4) by 7°·1. The temperature of the river was increased, at the commencement of the inundation in June, by the freshes from Abyssinia from 79° to 80°·5. The observations were taken at Thebes, immediately preceding, and immediately after, the appearance of the turbid milky hue that announces the periodical arrival of Egypt's great benefactor.

The Ganges, though having its source amid the snows of the Himálaya, and pursuing an opposite course to the Nile, that is, a course from northerly latitudes towards the equator, has a mean temperature, as it approaches the ocean, higher than that of the country on its banks. Its mean, between Calcutta and the sea, obtained from a great number of observations by Mr. G. PRINSEP, is stated not to be less than 81° FAHR.! while that of Calcutta does not exceed 78°. The Ganges, it is well known, is little indebted to the melting of the snows near its sources, but derives its waters chiefly from the periodical rains that fall near the borders of, and within, the tropics, between 30° and 22° N. lat. During the inundation, its waters in the lower parts of Bengal are spread over a superficies of alluvial soil and sand, more than 100 miles in breadth, the greater part of which has been parched by the droughts prevalent between the monsoons.

In order to obtain a better idea of the degree of heat absorbed and given out by the alluvium of the Nile, the sands and rocks in the beds of the rivers of India, I made the following observations.

In July 1840, a thermometer placed on the dark alluvium, then quite dry, of the Nile opposite the pyramid of Meydún at $12\frac{1}{2}$ P.M., having its bulb covered 0·1 of an inch with the same alluvium, stood at 136°·5. With, and on, the sand of the desert on the verge of the inundation line, at the same hour, it stood at 121°·5. The temperature of the air at the time, five feet above the surface of the dark alluvium, was 105°·5: the same height above that of the desert, it was 103°·5; sky unclouded. Although the surface of the sands during the clear serene nights of Egypt is cooled considerably by radiation, still a little below the surface they retain a great portion of the solar heat. In July, at sunrise, the surface of the sandy desert, on the banks of the Nile at Thebes, lat. 25° 26′ N., which during the heat of the day indicated a temperature of 130°, had cooled down to 69°·5, while the thermometer a foot *below* the surface stood at 83°: temperature of the air 75°.

The temperature of the granite rocks in the beds of the Toombuddra and the Kistnah, during the months of May and June, at 2 P.M., I found from 118° to 120°: during the night they cooled down usually to 83°. The temperature of the surface sands in these rivers was slightly higher than that of the granite.

The temperature of rivers whose supply, like those in South India, depends more on the periodical rains than on springs, is consequently influenced by the temperature of the former. That of the monsoon showers, which fell on the western coast near Mangalore during the months of May and June, varied from 73° to 79°, affording a mean of 76°. The rains falling on the elevated table-land of the Ceded districts, from June to December, ranged from 71°·5 to 79°·5, giving the mean 75°·5. The mean general height of the plain, between lat. 13° and 17° N., is 1300 feet above the sea's level. The temperature of the showers was invariably modified by the conditions affecting that of rain water in extra-tropical countries, namely, the elevation at which condensed, and the temperature of the atmospheric strata through which the showers fell.

The temperature of the Brahmapútra river at Sadya in Assam, was found in September by Mr. Griffiths to range from 63° to 70°. That of the air above the river, from 68° to 100°. That of the Indus, by Gerard, in March, near Attock, was 32°.

Temperature of the Ocean on the Equator and between the Tropics.—The influence of the trade-winds, cold currents from high latitudes, frequent showers, evaporation, &c., contribute to cool the air and surface of the ocean at the equator. The extremes of the temperature of the latter, at great distances from land, have been pretty correctly stated by M. Arago at 80°·8 and 84°·2. On crossing the line in the Atlantic Ocean (in long. 20°·7 W.) I found the temperature of the sea 84°·5; air in the shade, 87°: in the Indian Ocean (long. 58°·54 E.) 81°·5; air in the shade 82°·5. In the

same oceans, near the land, and in narrow seas, the range between the extremes is much greater than 3°·4. In the Red Sea, from the Straits of Babelmandel to the tropic of Cancer, I found it, in the month of May, to be 6°, viz. from 82° to 88°; and in the Indian Ocean, from lat. 12° to 19°, so much as 8°·5, viz. from 78° to 87°·5. In the Straits of Malacca, in lat. 2° N., it ranged from 80° to 85°·2.

On some parts of the west coast of India (where 123½ inches of rain falls during the year), during the monsoon, the surface of the sea is considerably cooled by the freshes from the numerous rivers and streamlets that descend from the lofty mountains of the Ghauts. Off Honáwer, lat. 14° 16′ N., the temperature of the sea during the dry season was 85°·5. During the monsoon it fell to 79°; average temperature of rain water at the time, 75°·7; of rivers, 76°. From its inferior specific gravity, the fresh muddy water from the hills floats on the surface of the sea to considerable distances, without being intimately blended. In the depth of the monsoon, near Mangalore, in 1839, the water was observed to be nearly fresh a mile off the coast; and I have seen the Mediterranean discoloured by the turbid inundation of the Nile to a distance of nearly forty miles from the Damietta embouchure.

Mean Temperature in India.—Colonel SYKES, in his statistics of the Deccan, has already noted one remarkable feature touching the mean temperature of places at elevations on the table-land of India, namely, that it is much higher than the mean for the same places, calculated agreeably to MAYER's formula. To the instances he has cited of this fact, of places on the plateau of the Deccan, may be added the following, occurring on the table-land of South India.

Places.	Feet above sea.	Lat. N.	Observed mean.	Calculated mean.	Difference.
		° ′	°	°	°
Hydrabad ..	1720	17 15	80	74·72	5·28
Nagpore	1101	21 10	80	74·26	5·74
Bellary.....	1600	15 5	80·5	76·12	4·38
Bangalore ..	3000	12 57	74·39	73·05	1·34
Seringapatam	2412	12 25	77·06	74·93	2·13

Among the principal causes of this differential height of temperature,—a difference more remarkable when compared with the indications afforded by the improved formulæ of BREWSTER, D'AUBUISSON and ATKINSON,—may be enumerated the physical aspect and extent of the elevated plains on which these places stand,—the rapidity with which the drainage water passes off, and consequent little evaporation,—the comparatively flat, or gently undulating surface,—its bareness of vegetation during great part of the year,—the non-influence of alternations of land and sea breezes, by which places near the sea are cooled,—the partial influence of the monsoon and scantiness of rain,—the favourable conditions of the atmosphere for irradiation, and the capacity of the soil for imbibing and giving out the solar heat. The temperature of the granitic soil in the vicinity of Bellary, at 2 P.M., in May, reached 121°; that of the Régur, or black soil, 122°·5: the temperature of the air in the shade, 95°·5: at midnight the temperature of the black soil was still so high as 86°; temperature of the

air 80°. That of a bare rock of granite, the same locality, at 2 P.M., was 120°·5 ; of black basaltic rock 122°. The temperature of the granite at midnight was 86°·5. Both Bellary and Hydrabad are situated under the shade almost of bare granitic masses, in the midst of plains covered with sheets of the granitic and black régur soils just alluded to, whose almost treeless extent during the hot months is shrunk up and intersected by deep and countless fissures. The climate of the former station is nearly as dry as that of Egypt. In 1838 only 11·25 inches of rain fell during the year. The atmosphere is remarkable for transparency and freedom from clouds. The foregoing views appear to be strengthened by the fact, that the observed mean temperature of the elevated stations of Ootacamund (7221 feet above the sea's level), Mercára (4500 feet), and Candy in Ceylon (1680 feet), are *lower* than their calculated mean temperatures. The calculated mean of Ootacamund is 61°·64, observed mean 55°·8; of Mercára 68°·99, observed mean 65°·58; and of Candy 78°·58, observed mean 73°·3. Now all these places are surrounded by an irregular surface of hill and valley, generally clothed with eternal forest, presenting an extensive radiating and evaporating surface, and shading the drainage of heavy monsoons that lingers in their swampy hollows. The humidity of the atmosphere at these stations is very great; at Mercára, during nearly half the year, its hygrometric condition closely approaches saturation. Hence, favoured by the alternations of land and sea breezes, even close to the sea's level, the low temperatures of some places near the equator, viz. Singapore, lat. 1° 15′ N., mean temperature 80°·7; Malacca, lat. 2° 14′ N., mean temperature 80°·4; Penang, lat. 5° N., mean temperature 80°·5; Province Wellesley, lat. 5° 20′ N., mean temperature 79°·5. The monsoons are distributed over these forest-clad regions of the equator in an almost daily succession of refreshing showers throughout the year. May not the vital functions of the plants, covering large tracts of country, particularly those concerned in their respiration and nutrition, exert an influence in cooling over-heated states of the atmosphere?

It may be further stated, in corroboration of the high temperature of table-lands being mainly produced by the causes referred to above, that the temperature of isolated peaks and summits of ridges, rising with a rapid ascent and confined superficies from their elevated level, appears to diminish in a greater ratio than 1° FAHR. for every 352 feet of ascent; when, perhaps, that of the aggregate height from the sea's level is in strict accordance with this rule. The mean of a month's observations by Lieut. CAMPBELL, at the summit and base of the rock of Raya-Cottah on the table-land of Mysore, above which it is elevated 500 feet, gave a decrease of temperature amounting to 3°·35. The diurnal mean difference between the temperature of the summit of a mountain on the table-land of Bellary, and that of the plain at its base, I found so great as 7°·5 for the 1500 feet of elevation which separates them. This table-land has a mean temperature of nearly 4°·5 above its calculated mean. The difference of temperature of two wells, one at the summit of Mount Sinai, and the other 2000 feet below, amounted to 6°, a result closely approximating that of the comparative observations at Geneva and St. Bernard.

The highest known mean temperature of any place in India is that of Pondicherry, which, though this city stands only a little more than a degree to the south of Madras, is stated to reach 85°·28. That of Madras, in lat. 13° 5′ N., is 80°·42, and of Columbo, more than 5° nearer the equator than Pondicherry, only 80°·75. I am not aware that any reason has been assigned for this extraordinarily high mean temperature; the lower temperature of some wells in the vicinity of Pondicherry leads me to doubt its correctness.

BOUSSINGAULT'S *Mode of ascertaining the Mean Temperature of Tropical Countries.*—An expeditious mode for ascertaining the approximate mean temperature of equinoctial regions has been proposed by M. BOUSSINGAULT, and recommended to travellers, on occasions where time and opportunity do not admit of the usual means. I hardly need remark, that this method is grounded on the hypothesis, that between the tropics the temperature of the earth's crust is constant at the depth of about a foot (one-third of a metre) beneath its surface, and consists in sinking a thermometer in the soil perforated to this depth, under sheds, huts of natives, or other spots sheltered from direct warmth produced by absorption of the solar heat, from nocturnal radiation, and from the infiltration of rain water. The result of my own experiments in India indicates that the soil at the depth of a foot is subject to an annual, and, in light soils, to a diurnal fluctuation, varying according to the intensity of the sun's rays on the soil surrounding the sheltered spots where the experiments were conducted; and radiation modified by the dry and open, moist and close nature of the soil. During cloudy weather these fluctuations were consequently found at their minimum. The maximum of diurnal fluctuation observed was at Bellary, on the centre of the table-land of peninsular India, in lat. 15° 5′ N., and 1600 feet above the sea's level; mean temperature about 80°·5. The experiments were made in the hot month of May, sky unclouded; the soil was reddish and light in texture, and completely sheltered by a thatched roof. Every precaution enjoined by M. BOUSSINGAULT was carefully attended to, and fresh holes bored every day.

EXPERIMENT.—*First Day.*

	Earth.	Air in Shade.
	°	°
Sunrise . . .	86·5	81
2 P.M.	91·3	96·5
Second Day.		
Sunrise . . .	85	78
2 P.M.	89	92
Third Day.		
Sunrise . . .	85·5	78·5
2 P.M.	90	95
Fourth Day.		
Sunrise . . .	87	75
2 P.M.	89	92

At Cassergode, on the west coast, lat. N. 12° 29′, whose mean temperature is about 80°, the diurnal fluctuation amounted to only 1°·5 in cloudy weather. At Mangalore, on the same coast, lat. N. 12° 53′, it amounted on a clear day to 2°·75. The last experiment was made, at my request, by my friend Mr. B. G. Maurice, Madras Medical Service. In stiff clayey soils, at the depth of four feet from the surface, and sheltered to the distance of six yards radius from the spot perforated, the temperature fluctuated but little, and gave a tolerably correct mean of the air. In light sandy soils a greater depth is necessary; and at all times it is advisable to observe the temperature of the perforation in the soil at the coldest and hottest periods of the day, which, with an unclouded sky, will be found to occur at, or just before, sunrise, and from 2 to 3 P.M. Such observations should, if possible, be compared with the temperature of a spring or well of moderate depth, at from six to ten feet below the surface, bearing in mind what has already been stated regarding the causes affecting the temperature of wells and springs.

Temperature of Springs and Wells.

Names of Places.	Position in			Formation in which situate.	Depth to surface of water.	Depth of water.	Temperature of air in shade at time of observation.	Temperature of water.	Approximate annual mean temperature of air.	Months of observation.	Remarks.
	Lat. N.	Long. E.	Approximate height in feet above the sea.								
	° ′	° ′			ft.	ft.	° ′	° ′	° ′		
Alexandria	31 13	27 35	Shore.	Limestone.	20	6	79 5	74 5	.....	July.	Large ancient reservoirs underneath the old city, communicating with the surface by narrow shafts, fed by Nile at high water.
Cairo	30 2	28 58	50	Limestone.	21	8	80 0	76 0	72 4	July.	Water brackish; shaft three feet in diameter.
Thebes	25° and 26°	30° and 31°	.....	Limestone.	10	5	82 0	79 0	.....	June.	Water brackish; shaft three feet in diameter.
Mount Sinai (summit)	28 33	34 0	7000	Granite.	16	9	75 0	55 0	.....	June.	Rain-water collected in deep cavity on summit. Ruppell makes summit 7035 French feet. Moresley 5956 English feet. Mean of two observations by Wellsted, 7505; 2500 above Convent. Three last are springs from three to four feet in diameter.
Fount of Elias (ascent of Mount Sinai	.....	.....	6000	Granite.	14	7	73 4	58 5	.....	June.	
Spring of Mariam (ascent)	.....	.....	5500	Granite.	1	2	79 2	59 5	.....	June.	
Well at foot of Mount Sinai	.....	.....	5000	Granite.	4	20	79 0	61 0	.....	June.	Narrow shaft, about four feet in diameter.
Wells of Tor	28 15	33 38	80	Limestone.	4	1½	84 0	77 2	.....	June.	About four feet in diameter; possibly thermal.
Wells of Ambajeh (Egyptian Desert)	26° and 27°	31° and 32°	200	Limestone.	1	½	84 0	82 5	.....	June.	Spring on surface. Saline, muriate and sulphate of soda.
Wells of Hammamet (Egyptian Desert)	26 10	31 0	700	Sand and sandstone	130	½	85 0	76 2	.....	June.	Brackish. Shaft two or three yards in diameter.
Bombay	.....	.....	Shore.	Trap.	10	5	82 0	79 5	.....	April.	Narrow shaft, four feet in diameter.
Beder	17 49	77 46	2300	Laterite.	200	6	82 0	79 0	77 5	June.	Narrow shaft, four feet in diameter.
Calliany	17 50	77 5	.....	Laterite.	35	10	89 0	78 5	.....	July.	Narrow shaft, four feet in diameter.
Hydrabad	17 15	78 35	1720	Granite.	4	10	87 5	79 5	81 0	June.	Large open spring of pure water, ten or twelve yards in diameter.
Hydrabad	17 15	78 35	1720	Granite.	20	12	87 0	80 0	81 0	June.	Deep wells of pure water, narrow shaft.
Wells between the Kistnah and Hydrabad	15° to 17° 50′	.....	900 to 1720	Granite.	6 to 30	6 to 10	84° to 87°	78° to 80°	79 81	June.	Wells of pure or slightly brackish water.
Gokauk	16 11	74 58	.....	Sandstone.	10	4	78 5	75 5	.....	July.	Shaft four feet in diameter.
Darwar	15 20	75 8	2400	Clay-slate.	12	8	79 3	75 3	75 0	July.	Shaft four feet in diameter.
Ceded districts	.....	.....	500 and 1600	Sandstone. Limestone. Granite.	.....	.....	.....	.....	78 5	July.	This is the average temperature of twenty-one springs and bouries between lat. 13° and 17° N., taken at all seasons of the year.
Udigherry	14 52	79 22	.....	Sandstone.	10	5	82 0	78 5	.....	January.	Shaft five feet.
Bunwassy	14 32	75 8	.....	Laterite.	2	1	72 5	75 8	.....	July.	Open bourie.
Nellore	14 28	80 3	100	Laterite.	5	2	82 0	78 5	.....	January.	Shaft five feet.
Kistnapatam	14 25	80 9	Shore.	Laterite.	10	4	82 2	80 0	.....	January.	Shaft five feet.
Honawer	14 16	79 56	Shore.	Laterite.	4	6	78 0	79 0	78 5	July.	Open well, about four yards square.
Madras	13 5	80 22	Shore.	Granite.	6	12	80 8	80 6	80 42	February.	Shaft five feet in diameter.
Mangalore	12 52	74 52	Shore.	Laterite.	3	20	80 0	80 75 and 81 0	80 21	June.	Shaft six feet in diameter.
Beypoor	11 11	75 53	Shore.	Laterite.	20	6	83 4	81 3	80 5	March.	Shaft four feet in diameter.
Malacca	2 14	102 0	Shore.	Laterite.	40	8	84 0	80 5	80 4	June.	Spring, about three feet square.

Temperature of Rivers.

Names of Places.	Position in			Formation in which situate.	Depth to surface of water.	Depth of water.	Temperature of air in shade at time of observation.	Temperature of water.	Approximate annual mean temperature of air.	Months of observation.	Remarks.
	Lat. N.	Long. E.	Approximate height in feet above the sea.								
	° ′	° ′	ft.			ft.	° ′	° ′	° ′		
Cauvery	12 20	76 47	2412	Gneiss.	.....	12	85 0	77 5	77 06	May.	
Pennaur	14 28	80 3	100	Laterite.	.....	6	82 4	79 5	.....	January.	
Gaorsippa river	14 16	75 45	.....	Gneiss.	.....	15	72 5	75 5	.....	July.	
Gaorsippa at sea	14 16	75 25	Shore.	Laterite.	.....	10	78 0	78 5	.....	July.	
Rivulets of Western Coast	13° and 14°	75 0	Shore.	Laterite.	.....	.....	77 5	78 0	.....	July.	
Tambuddra	15 44	78 2	880	Limestone.	.....	12	85 0	79 0	.....	May.	Swollen and muddy by monsoon; January coldest month, its temperature was 69° 5′.
Hendri	15 44	78 2	880	Limestone.	.....	10	83 0	78 5	.....	June.	Swollen and muddy by monsoon; January coldest month, its temperature was 69° 5′.
Kistnah	15 57	78 2	910	Limestone.	.....	15	89 0	79 0	.....	June.	Swollen and muddy by monsoon; January coldest month, its temperature was 69° 5′.
Gokauk	16 11	74 56	2800	Sandstone.	.....	12	82 0	75 6	.....	July.	
Birna	17 7	.....	1730	Limestone.	.....	.....	.....	78 0	.....	June.	
Godavery	18 0	.....	130	Granite.	.....	14	79 0	74 5	.....	June.	
Indus, near Attock	.....	.....	.....	.....	.....	.....	.....	32 0	.....	March.	GERARD.
Brahmapútra, in Upper Assam	.....	.....	.....	.....	.....	.....	68° and 100°	63° and 70°	.....	Sept.	GRIFFITHS.
Ganges	21° and 23°	.....	.....	Alluvium.	.....	.....	.....	81 0	78 0	Annual mean.	G. PRINSEP.
Nile	25 and 30	.....	.....	Limestone.	.....	.....	81 0	79 5	.....	June & July.	

Temperature of Thermal Springs.

Names of Places.	Position in		Approximate height in feet above the sea.	Soil on rock in which situate.	Depth to surface of water.	Depth of water.	Temperature of air in shade at time of observation.	Temperature of water. Fahr.	Approximate mean annual temperature of air.	Months of observation.	Remarks.
	Lat. N.	Long. E.									
			ft.		ft.	ft.	°	° ′			
Jauri on Sutledge ...	30° and 32°	.....	.....	.....	.....	.....	.....	130 0	.....	.....	Hamilton. Sulphurous.
Jumnotri	30° 52′	.....	10,849	Granite.	.....	.....	.....	194 0	.....	.....	Hodgson. Ferruginous.
Muktinath	29 9	83° 18′	.....	.....	.....	.....	.....	.....	.....	.....	
Bowitti (Oasis parva, Egypt)	28° and 29°	27° and 28°	300	Sandstone.	.....	.....	.....	92 75	.....	.....	Wilkinson.
El Kasr (Oasis parva, Egypt)	28 and 29	27 and 28	300	Sandstone.	.....	.....	.....	93 5	.....	.....	Wilkinson.
Sonah, near Delhi ...	28 and 29	76 and 77	.....	Sandstone.	.....	.....	.....	108 0	.....	.....	Ludlow.
Hummám Músa (Wells of Elim?) near Tor.....	28° 5′	33° 38′	60	Limestone.	2	2½	89	91 6	.....	June.	•
El Kasr in Oasis of El Dakhleh	26° and 27°	27° and 28°	250	Limestone.	.....	.....	.....	102 0	.....	.....	Wilkinson.
Sitakhund	25 and 26	86 and 87	.....	Quartz.	.....	.....	.....	140 0	.....	.....	Adam. Tasteless, clear, gaseous; gas not ascertained.
Bheembund.....	24 and 25	86 and 87	.....	.....	.....	.....	.....	144 0	.....	March.	Buchanan.
Rajaghiri	24 and 25	85 and 86	.....	.....	.....	.....	70	108 0	.....	October.	Fullarton, and As. Journal, vol. iii. Saline.
Hazarybaugh	24 and 25	85 and 86	.....	.....	.....	.....	.....	108 0	.....	.....	H. Wilson and Fullarton. Sulph., hyd., mur., and sulph. of soda.
Hazarybaugh	24 and 25	85 and 86	.....	.....	.....	.....	.....	170 0	.....	.....	
Hazarybaugh	24 and 25	85 and 86	.....	.....	.....	.....	.....	190 0	.....	.....	
Kutkunsandy	23 and 25	86 and 87	.....	Trap and Granite.	.....	1	41	114 0	.....	.....	Fullarton.
Tantotya, near Pachete Hills.....	23 and 24	86 and 87	.....	.....	.....	.....	.....	190 0	.....	.....	Bell. Slightly chalybeate.
Kyrie	22 and 23	78 and 79	.....	Trap.	.....	.....	86	114 0	.....	February.	Spilsbury. Sulphurous, nearly pure water.
Maljihir	22 and 23	78 and 79	.....	Trap.	.....	.....	92	134 0	.....	February.	Spilsbury. Sulphurous, nearly pure water.
Devaki, Unei	20 and 21	73 and 74	.....	Trap (?).	.....	.....	.....	111° & 120°	.....	.....	Dr. White.
Kais	19° 59′	78° 58′	.....	Trap and Limestone.	.....	.....	81° to 100°	87 0	.....	June.	Malcolmson. Carbonic lime and carbonic acid gas.
Baugha.....	18 12	81 0	200	Sandstone and Limestone.	.....	.....	.....	110 0	.....	.....	Voysey.
Gondála	18 0	81 17	130	Granite and Trap.	.....	.....	70	120° & 139°	.....	.....	Voysey.
Allayen (Moulmein)..	16° and 17°	98 0	.....	Limestone.	.....	.....	.....	137 0	.....	December.	Foley.
Bhuga, near Cuddapul	14 and 15	78° and 79°	560	Sandstone.	2	2½	65° and 90°	88 0	79° 5′	December.	
Spring, near Salem ...	11° 37′	78° 13′	2000	Gneiss and Hornblende slate.	2	3	.....	84 0	.....	March.	Fischer.
Cannia (Ceylon), seven springs.....	8° and 9°	81° and 82°	.....	Quartz.	.....	.....	77	86° & 107°	.....	.....	Davy.
Sabung, Malacca.....	2 and 3	102 and 103	500	Granite.	Surface	3	105° in sun.	130 0	80 5	July.	
Ayer Pannas, Malacca	2° and 3°	102° and 103°	400	Granite.	Surface	2½	89°	113° 5′ and 120°	80 5	March.	

Comparative Register of the Temperature of the Air (in the shade) and of the Sea, from Bombay to Suez. The indications of thermometer are adjusted to those given by the standard of the Royal Society.

Month.	Lat. N.	Long. E.	Noon.		Midnight.		Remarks.	
			Air.	Water.	Air.	Water.		
May 1.	18° 36′	71° 41′	No obs.		No obs.		Indian and Arabian Seas.	
2.	18 5	69 38	87·5	85·5	86·5	84·5		
3.	17 30	67 28	87	86·5	87	No obs.		
4.	16 57	69 12	86	84	86·5	No obs.		
5.	15 29	63 5	87·5	85·5	85	85		
6.	15 52	60 53	87	85·5	85	84·5		
7.	15 19	58 40	85	83	86	84·5		
8.	14 56	56 17	85·5	82·5	85·5	82·5		
9.	14 27	53 56	85·5	84·5	83·5	84		
10.	13 43	51 39	86·5	83·5	84·5	84·5		
11.	13 32	48 43	88	85·2	87	85		
12.	12 46	45 53	89	86·6	87	85·5	Off coast of Arabia.	
13.	Aden.	Aden.	89·5	87·8	86·5	86	Back Bay, at anchor.	
14.	12 49	43 21	88·7	86	85·5	84·5		
15.	15 14	41 55	88	86	84	84	In sounding, off volcano of Gevel Teer.	
16.	17 28	40 20	87·5	87	87	84·5	Sun's rays 115° 2 P.M.	Red Sea.
17.	19 55	38 52	90	88	85	85·5	Sun's rays 120°·5 2 P.M.	
18.	22 23	37 21	86	84·5	85·5	82		
19.	24 38	36 13	84·2	80·5	82	79		
20.	26 38	34 45	84	80·5	89·5	82	Suez, hot khamsin set in about 10 P.M.	

Memoranda supplied by the kindness of a friend from the register kept on board the Honourable East India Company's Steamer Cleopatra, from Bombay to Suez.

Month.	Lat. N.	Long. E.	A.M.		P.M.		Remarks.
			Air.	Water.	Air.	Water.	
April 2.			89°	84°	81°	78°	
3.			84	83	80	78	
4.			84	84	81	83	
5.			81	83	81	83	
6.			80	82	80	82	
7.			No entry.		No entry.		
8.			No entry.		No entry.		
9.			No entry.		No entry.		
10.			No entry.		No entry.		At Aden.
11.			No entry.		No entry.		Passed Straits of Babelmandel.
12.			86	84	86 5	84	
13.			87	84	81	82	
14.			79	82	79	80	
15.			No entry.		No entry.		
16.			84	76	73	71	
17.			75	82	No entry.		Passed Island of Shadwan.
18.			No entry.		No entry.		Suez Bay.

N.B. The latitude and longitude have been omitted in the above register; but after making allowance for the more rapid run of the Cleopatra than that of the vessel in which I left India, and calculating from Bombay to the Straits of Babelmandel, and thence to Suez, an approximation may be made to the vessel's situation at the time of taking the observations. The indications could not be adjusted to the standard thermometer of the Society.

Note on the Thermal Springs of the Peninsula of India.

Since my arrival here my friend Mr. MALCOLMSON has put into my hands the first volume of the Bombay Medical and Physical Transactions, where I find, p. 257, a few notes on the thermal springs in the Konkan, by A. DUNCAN, Esq. The geographical distribution of these springs corroborates the remark in my paper under the head of thermal springs, viz. "that the majority of the springs termed thermal occur in India at or near lines of great faults." The thermal springs mentioned by Mr. DUNCAN lie at the base of the Western Ghaut elevation, intermediate between the mountains and the sea, generally from sixteen to twenty-four miles, or thereabout, inland from the latter. The line of springs follows pretty nearly that of the mountains, viz. nearly north and south, and extends from the vicinity of Surât, or about 21° N. lat. to South Rajapore: they are supposed to exist still further south, following at irregular intervals the line of West Ghauts to Ceylon. Not less than twelve are known to exist between Dasgaun and South Rajapore, viz.—

1 at Oonale in the taluk of Viziadroog.
3 in the Rutnaghirry taluk, at Rajwaree, Tooril and Sungmairy.
1 at Arowlee in the Konedree taluk.
1 at Mat, Hatkumbee Mahal.
1 at Oonale, in the Natoe Palivan Mahal, Severndroog.
3 at Oonale, Jaffrabad Mahal.
1 at Savi, in the Ryghur taluk, Bhar Nergannah.
1 at Oonale, Sankse taluk, Mahal Palee.

12 total.

Oonale is the native term for a hot spring. The temperature of all the springs examined exceeded, with a single exception, 100° FAHR., and amounted to 109°. That of Tooril, which unfortunately was not thermometrically ascertained, appeared to Mr. DUNCAN to be almost at the boiling-point. The water was not found to be mineral, though impregnated with sulphuretted hydrogen. A little higher up, on the hill where the thermal spring No. 1 occurs, is a singular intermittent cold spring, over which a temple has been built. It is resorted to by crowds of Hindus during the season when the fountain periodically flows, viz. during the hot months. A more minute analysis of the water, and a more continued series of thermometric observations, are a great desideratum.

The temperature of a hot spring of Oonye in the jungle between Bansda and Boharee is asserted by the Brahmins to diminish annually at the time of the full moon in April, so as to admit of persons bathing in it at this period, when the natives assemble there in great numbers for that purpose. This assertion was contradicted by the late Dr. WHITE, but the question, I see, has again been raised by the observations of Mr. J. S. LAW, of the Civil Service, who found the temperature of the hottest part of the spring to have diminished at this period from 124° to 94° FAHR. It is probable however that future observations on this supposed singular annual variation will set the matter at rest.

Bombay, July 15, 1842.

III. *An Account of* NEWTON'S *Dial presented to the Royal Society by the Rev.* CHARLES TURNOR, *in a letter addressed to the* MARQUIS OF NORTHAMPTON, *Pres. R.S., &c. By the Rev.* CHARLES TURNOR, *F.R.S. Communicated by the President.*

Received May 25, 1844,—Read June 13, 1844.

MY LORD,

YOUR Lordship having been pleased to express a wish to Captain SMYTH that I should furnish a detailed account of the Newtonian Dial which I have had the honour of presenting to the Royal Society, I beg to submit to your Lordship the following particulars. The dial was taken down in the early part of the present year from the south wall of the Manor House at Woolsthorpe*, a hamlet to Colsterworth in the county of Lincoln, the birthplace of NEWTON.

The house is built of stone, and the dial, now in the possession of the Royal Society, was marked on a large stone in the south wall at the angle of the building, and about six feet from the ground, and which was reduced to its present dimensions for the convenience of carriage. The name of NEWTON, with the exception of the first two letters, which have been obliterated by the hand of time, will, on close inspection, appear to have been inscribed under the dial in rude and capital letters. There is also another dial marked on the wall, smaller than the former, and not in such good preservation. The above are the only dials about the house which I have been able to discover, nor can I find by inquiry on the spot that more ever existed, though some of NEWTON's biographers assert that there were several. An opinion has always prevailed that the dials now in being were executed by NEWTON's own hand when a boy, which appears probable from the well-known fact, that at a very early period of his life he discovered a genius for mechanical contrivances, evinced more particularly by the construction of a windmill of his own invention, and a clock to go by water applied to its machinery. Finding, however, this latter contrivance (however ingenious) to fail in keeping accurate time, it is not improbable, that with a view to secure that object, he formed with his own hands the two dials in question; and very probably the dial now remaining in the wall of the house, from its inferiority in point of construction to that now in the possession of the Royal Society, was his first attempt in dial-making. The gnomons of these dials have unfortunately disappeared many years, but as they are described in some of the printed

* See Woodcut in the next page.

accounts as *clumsy* performances, it may be concluded that they were not the work of a professed mechanic, but were probably formed and applied by NEWTON himself when he constructed the dials.

I trust your Lordship will allow me to express the high satisfaction I feel in seeing this very interesting relic in the possession of that Society of which NEWTON was so distinguished an ornament, and over which he presided more than twenty years.

I must beg your Lordship's permission to add, that for the gratification which I experience on this occasion, I am greatly indebted to my nephew, CHRISTOPHER TURNOR, Esq., of Stoke, Rochford, to whom the manor-house and landed property of NEWTON now belong, and who not only permitted, but kindly encouraged me to offer this valuable relic to that Society, which he, as well as myself, consider as its fittest and most appropriate depository.

I have the honour to be, my Lord,

Your Lordship's obedient humble Servant,

CHARLES TURNOR.

Spa Buildings, Cheltenham,
May 24, 1844.

MANOR-HOUSE, WOOLSTHORPE;

THE BIRTH-PLACE OF

SIR ISAAC NEWTON, P.R.S.,

SHOWING THE SOLAR DIALS WHICH HE MADE WHEN A BOY.

IV. Ἀμόρφωτα, No. I.—*On a Case of Superficial Colour presented by a homogeneous liquid internally colourless.*

By Sir John Frederick William Herschel, *Bart., K.H., F.R.S., &c. &c.*

Received January 28, 1845,—Read February 13, 1845.

A CERTAIN variety of fluor spar, of a green colour, from Alston Moor, is well known to mineralogists by its curious property of exhibiting a superficial colour, differing much from its transmitted tint, being a fine blue of a peculiar and delicate aspect like the bloom on a plum, and like that bloom might perhaps be referred to a peculiar texture of the surface, the result of crystallization, were it not that it appears equally on a surface artificially cut and polished. Glasses also are manufactured which, by the agency of a delicate superficial film, consisting apparently of a dull green-coloured powder, and reflecting (or rather dispersing) a green light, exhibit a brownish red tint by transmission; chloride of sulphur, and the infusion of lignum nephriticum are particularized in some books as exhibiting different colours by transmitted and reflected light. As respects the chloride of sulphur, the statement is incorrect, and has originated in a misapprehension of its scale of absorbent action, which (as is the case with many dichromatic media) causes its hue to change from green to red by mere increase of thickness. In the infusion of lignum nephriticum, and in one other instance which has occurred to my notice, the reflected tint arises from suspended particles too minute, or too nearly of the specific gravity of the liquid, to be separated by *subsidence**, the transmitted colour being that of the transparent liquid in which they float, and the particles themselves being opake.

The case which I am about to describe is not precisely parallel to any of these, though far more striking than either. That of the fluor spar presents the closest analogy to it, though from what we know of the impracticability of obliterating the internal structure of mother-of-pearl by any artificial polish, the difference between the solid and fluid states of aggregation precludes any argument from that phenomenon to the one in question.

The sulphate of quinine is well known to be of extremely sparing solubility in water. It is however easily and copiously soluble in tartaric acid. Equal weights of the sulphate and of crystallised tartaric acid†, rubbed up together with addition of a very little water, dissolve entirely and immediately. It is this solution, largely diluted, which exhibits the optical phenomenon in question. Though perfectly trans-

* I write from recollection of an experiment made nearly twenty years ago, and which I cannot repeat for want of a specimen of the wood. I think the *filtered* liquid did *not* exhibit the double colour.

† Citric acid answers equally well.

parent and colourless when held between the eye and the light, or a white object, it yet exhibits in certain aspects, and under certain incidences of the light, an extremely vivid and beautiful celestial blue colour, which, from the circumstances of its occurrence, would seem to originate in those strata which the light first penetrates in entering the liquid, and which, if not strictly superficial, at least exert their peculiar power of analysing the incident rays and dispersing those which compose the tint in question, only through a very small depth within the medium.

To see the colour in question to advantage, all that is requisite is to dissolve the two ingredients above mentioned in equal proportions, in about a hundred times their joint weight of water, and having filtered the solution, pour it into a tall narrow cylindrical glass vessel or test tube, which is to be set upright on a dark-coloured substance before an open window exposed to strong daylight or sunshine, but with no cross lights, or any strong reflected light from behind. If we look down perpendicularly into the vessel so that the visual ray shall graze the internal surface of the glass through a great part of its depth, the whole of that surface of the liquid on which the light first strikes will appear of a lively blue, which as the situation of the eye changes is either fore-shortened into a vivid concave gleam, or opens out into a paler and broader band, as the visual line is more or less oblique to the glass surface.

If the liquid be poured out into another vessel, the descending stream gleams internally from all its undulating inequalities with the same lively yet delicate blue colour, thus clearly demonstrating that contact with a denser medium has no share in producing this singular phenomenon.

The thinnest film of the liquid seems quite as effective in producing this superficial colour as a considerable thickness. For instance, if in pouring it from one glass into another, it be made to trickle down the internal surface of the receiving glass towards the light, or if instead of falling in drops from a filter, the end of the funnel be made to touch the internal surface of the vessel well-moistened, so as to spread the descending stream over an extensive surface, the intensity of the colour is such that it is almost impossible to avoid supposing that we have a highly coloured liquid under our view.

By candlelight the gleam is less vivid, and verges more to violet. Analysed by a prism the red rays are found to be almost entirely absent. No signs of polarization were perceived in it, on viewing it through a tourmaline plate turned round in its own plane.

As this phenomenon in all its circumstances is (so far as I am aware) unique in physical optics, I have thought no apology necessary for simply describing, without attempting to pursue it further, which present circumstances do not permit. It would be interesting to know whether the property in question is characteristic of quinine, or is participated in by cinchonine, salicine, or any of the other vegetable alkaloids, which I have not been able to decide for want of specimens.

J. F. W. Herschel.

Collingwood, Jan. 25, 1845.

Received February 20, 1845.

P.S.—Having been obligingly favoured by Professor DANIELL with specimens of very pure cinchonine and salicine, I am enabled to state that they do not possess in the smallest appreciable degree the curious property above shown to belong to quinine.

As regards the latter alkaloid, all the acids I have tried appear to produce the same effect, though not all in an equal degree. The muriatic seems least efficacious; the sulphuric and acetic decidedly the most so. The intensity of the superficial colour produced when either of the latter acids (very dilute) is used, is really surprising.

Only *acid* solutions succeed. After precipitating by excess of potash a solution of quinine, the liquid filtered was very bitter, and of course contained quinine. It however exhibited no trace of superficial colour; but on dropping powdered tartaric acid into the test glass the blue colour was instantly developed, and seen to follow the course of the descending acid.

J. F. W. H.

Feb. 16, 1845.

V. Ἀμόρφωτα, No. II.—*On the Epipŏlic Dispersion of Light, being a Supplement to a paper entitled, "On a Case of Superficial Colour presented by a homogeneous liquid internally colourless."* *By Sir* J. F. W. Herschel, *Bart., K.H., F.R.S., &c.*

Received March 6,—Read Apr'l 3, 1845.

In reasoning on the peculiar coloured dispersion operated on a portion of a beam of white light intromitted into a solution of sulphate of quinine, it occurred to me as a subject well worthy of inquiry whether the rays so selected for dispersion and thus singularly separated from the rest, were distinguished by any other peculiarity; whether in effect an *analysis* of the incident light into two distinct species *qualitatively* different had been performed, or merely a simple *subdivision*, such as takes place, for instance, in partial reflexion, as in the phenomena of the colours of thin plates. Another interesting subject of inquiry presents itself in the laws which regulate this singular mode of dispersion itself, which, for brevity, I shall venture to call (at least provisionally) *epipŏlic*, from ἐπιπολὴ, *a surface*, the seat of the dispersion being at or very near the intromitting surface.

As regards the question of analysis, two modes of examination present themselves, viz. either,—1, by subjecting the dispersed portion of the light to experiment, or, 2, the residual portion, which, having escaped dispersion, preserves the unity of its direction; and on that account, as well as by reason of its vastly superior intensity, offers itself more readily to experimental inquiry.

The colour of the dispersed portion being blue, that of the residual beam ought, of course, to verge towards orange. But owing to the large excess of undecomposed white light present, this tendency is inappreciable; and the regularly transmitted beam is not to be distinguished by the eye from white light. Another reason is, that some portion of the dispersed necessarily mingles with the regularly transmitted beam, the medium being equally *permeable* to both; so that in viewing an extensive white surface (the cloudy sky for instance, or a piece of white paper), the regularly transmitted ray reaching the eye in any given direction, that is, from any one point in the luminous surface, has, intermingled with it, a dispersed ray from every other point of that surface, the totality of which goes to restore to it some material portion of the blue light which it lost by dispersion at its intromission.

In the ordinary production of colour in liquids by absorption of the complementary tint, the smallest preference of one over the other coloured rays may be magnified and brought into evidence as a cause of coloration by increasing the thickness of the transmitting medium, or by passing the light successively through

many vessels filled with it. Accordingly it might be supposed that by passing the same incident beam successively through many such dispersive surfaces, the whole of the blue rays would at length be separated from it, and an orange, or red residual beam be left. But this is not the case, the reason of which is to be found in a very remarkable peculiarity in the transmitted light, which may be thus announced.

An epipolized beam of light (meaning thereby a beam which has been once transmitted through a quiniferous solution and undergone its dispersing action) *is incapable of further undergoing epipolic dispersion.*

In proof of this, the following experiments may be adduced.

Exp. 1. A glass jar being filled with a quiniferous solution*, a piece of plate-glass was immersed in it vertically, so as to be entirely covered and to present one face directly to the incident light. In this situation, when viewed by an eye almost perpendicularly over it, so as to graze either surface very obliquely, neither the anterior nor posterior face showed the slightest trace of epipolic colour. Now the light, at its egress from the immersed glass, entered the liquid under precisely the same circumstances as that which, when traversing the anterior surface of the glass jar, underwent epipolic dispersion on first entering the liquid. It had therefore lost a property which it originally possessed, and could not therefore be considered, *qualitatively*, the same light.

Exp. 2. The epipolic tint is developed only on the surface of incidence. When the solution is exposed to light in a glass vessel, the posterior surface, whether viewed internally or externally, is quite colourless. Here again, since ingress and egress into and out of a medium are, optically speaking, convertible, *a qualitative analysis* at the surface of incidence would appear to be indicated.

Exp. 3. A test cylinder filled to the height of two or three inches with the solution was set upright on black velvet, its bottom being also shaded to the depth of half an inch (to prevent reflected light from the bottom from reaching the eye). The epipolic tint being now fully developed, a hollow parallelopiped of plate glass, filled with the same solution, was interposed between the test cylinder and the incident light, side light being at the same time obstructed by screens duly placed. Immediately the epipolic colour in the interior of the cylinder vanished altogether. The transmitting vessel was now emptied of its contents and filled with pure water,

* The solution here used and subsequently referred to (except when otherwise expressed) is formed by adding to sulphuric acid, diluted to such an extent as just to bear being swallowed without pain, about one two-hundredth part of its weight, (the weight, i. e. of the *diluted* acid) of sulphate of quinine. When of this strength t is difficult to believe that a bottle half-filled with it contains a colourless liquid. When shaken, it tinges the glass vividly blue: lively blue gleams are reflected from the interior, and from the capillary ring at the surface level, &c. I may mention that in one instance a rose-coloured solution was formed, which I have never been able to reproduce. The ingredients were taken from the very same parcels which gave the usual colourless solution, and the mixture made in the identical vessel which had just recently served for the same experiments, and which had not even been washed, and from which a colourless solution had just been emptied. If owing to any foreign ingredient accidentally present, the quantity must have been inconceivably minute.

from which its former contents were in no way distinguishable by an eye situated behind it. Being then replaced as before, so as to intercept the light incident on the test cylinder, the epipolic colour was produced, exactly as if nothing had been interposed; a trifling difference of intensity only excepted, which arose from the glass used not being wholly devoid of colour.

Exp. 4. This experiment was varied so as to present a result disengaged from this slight source of uncertainty, and perfectly decisive. A cylindrical jar was coated externally with black paper round three-fourths of its circumference, as was also its bottom, and a ring of the same paper was carried round the cylinder at the bottom so as to cut off light from being internally reflected on its base. In it was set upright a test cylinder of the solution, and the jar was then filled with pure water rising considerably above the solution in the cylinder. When exposed to light as usual, the epipolic tint was finely seen. But on emptying out the water, and introducing in its stead an equal quantity of the quiniferous solution, the tint in question was completely destroyed, whether the surface of the cylinder was viewed from within or from without, proving evidently that no rays susceptible of epipolic dispersion had reached its surface. This result was rendered the more remarkable by an effect of contrast. The *external, upper* portion of the cylinder, above its liquid contents, but below the level of the liquid in the jar, reflected to the eye (or rather the air within it reflected) a pretty strong blue gleam, being no other than the epipolically dispersed light of the anterior surface of the liquid in the jar; while all below (being glass in contact with the liquid on both sides and so deprived of reflective power on both surfaces) was completely dark and almost invisible.

When the interior test cylinder was sloped backwards from the incident light at an angle of about 70° to the horizon, a beautiful and instructive feature was developed. In this situation of things, the interior liquid being as usual the quinine solution, and the exterior pure water; to an eye perpendicularly over the surface, the whole anterior portion of the cylinder from below upwards to the surface of the interior liquid, appeared coated as it were internally with a most delicate and beautiful blue film of extreme tenuity and perfect transparency, presenting a singular ghost-like appearance, easier produced than described. This being seen *through* the cylinder, by an eye situated externally to its prolongation, affords a proof that the epipolic dispersion takes place in all directions: but except in this mode of viewing it the rays dispersed outwards cannot reach the eye, or not in abundance (for which a very oblique incidence is required), being at such an incidence internally and totally reflected by the outer surface of the glass. To see this to advantage an eye-tube internally blackened should be used to guard the eye from extraneous light. Such a tube indeed is generally advantageous in all these experiments.

If, instead of water, the test cylinder be plunged into a solution of quinine, all else remaining as before, the blue film in question *totally disappears*. I tried a great many other liquids, all in fact which I had at hand in sufficient quantity and colour-

less, or but little coloured, in hopes of discovering something which might elucidate the subject. Strong alcohol, solution of corrosive sublimate, ammonia, &c. acted as water; allowing the blue film to be seen externally at a perpendicular incidence of the visual ray to the surface of the liquid. With strong sulphuric acid, and with muriate of lime so concentrated as to be syrupy, this was not possible, but the film became visible, and of its full intensity, on moving the eye forward (i. e. towards the incident light). When sulphate of manganese was used, its delicate pale rose-colour no way prevented a fine exhibition of the blue film (a point to which I shall have occasion to revert). On the other hand, the lemon yellow-colour of nitrate of uranium (a much fuller tint) materially enfeebled, though it did not prevent the formation of the film. This last effect did however appear to be produced by two liquids, viz. pyroxylic spirit in a small degree, and oil of turpentine in a much greater; the effect in this case being very obviously much more than could justly be attributed to a trifling tinge of yellow in the oil (which was not fresh), as I satisfied myself by a comparative experiment with water purposely coloured to a similar tint of greater intensity. Neither of these liquids however was found on trial in the test cylinder, or otherwise, to possess in the smallest degree the property of epipolic dispersion; nor have I found any other liquid which does so.

Exp. 5. Among solids the only one I am acquainted with possessed of a similar property, is the green fluor of Alston Moor, which exhibits by superficial dispersion a fine deep blue colour, very different from the inherent or absorptive colour of the mineral. This is strictly an epipolic tint, as the following experiment will show, and at the same time affords another, and not a little striking confirmation of the general proposition announced in p. 148. I should premise that to see the epipolic colour of the fluor in perfection, it must be laid on black velvet, or the reflexion of light from its posterior surfaces must be destroyed by roughening and coating them with black sealing-wax. In this state, if exposed to daylight at a window, and viewed through a blackened eye-tube, it is seen not as a green, but as a fine deep blue crystal.

If a piece of fluor so prepared be placed in water in a glass standing on black velvet, the blue epipolic colour is seen greatly heightened. But if the water be exchanged for a solution of quinine, this colour is completely destroyed and the surface appears simply black. To make the experiment successfully, the greatest care must be taken to cut off all lateral or reflected light. The arrangement I adopted was, to coat a fluor as above described, and fastening it with black sealing-wax to a wire, to lower it into the coated jar described in Exp. 4, filled alternately with a solution of quinine and with pure water. Using the eye-tube for further precaution, the destruction of the epipolic tint by the solution was quite as complete as if instead of the fluor a test glass full of the quiniferous solution had been used.

It would certainly appear from these experiments that the residual beam after undergoing epipolic dispersion had lost some constituent portion, or otherwise under-

gone some qualitative modification which might be considered as rendering it specifically different from the incident beam. It cannot be the mere tinge of *colour* which the loss of so small a portion of blue light has given to it. There is still plenty of blue light left, and the experiment on sulphate of manganese proves that a mere *absorption* of a much larger proportion of the blue rays has not the same effect. Moreover the portion of light *dispersed* traverses the solution of quinine with perfect facility, proving that no peculiar absorptive power *is* exercised by that medium on these rays; nor indeed would the separation of such rays by dispersion at the surface in any way tinge *the medium itself* with a complementary tint, but only the residual beam.

I come now to the examination of the dispersed portion of the light. As just remarked, when once *dispersed* it is *freely transmitted.* The epipolic colour is seen as well, in a long test-cylinder filled with the solution, at the bottom of the tube as at the top, when viewed by an eye situated in its axis, supposed vertical. If all light be cut off from the tube by a sheet of black paper rolled round it, except from the lowest inch of its length, that inch is seen to gleam with quite as intense a colour as when the uppermost inch only is so exposed.

I have already had occasion to remark that the epipolic tint is a compound one. To obtain a pure ray for prismatic analysis, a cylindrical glass jar with perpendicular sides was partly filled with the quiniferous liquid and placed in a strong light, the whole anterior side being coated with black paper rising somewhat above the level of the liquid. The eye was then placed in such a position, below that level, that the visual ray proceeding *from* it would suffer total reflexion at the under surface. For comparison, a similar vessel of water, similarly shaded, was placed beside it. The surface of this, so viewed from below, was of course perfectly black, no ray from above being able so to penetrate it as to reach the eye. Not so the quiniferous solution. In this the under surface was wholly visible, of a fine blue colour, considerably deeper in tint than in the ordinary mode of viewing it, though not of so rich and saturated a character as the epipolic blue of the fluor. It was, however, much more luminous, and being thus completely purified from all possible admixture of regularly refracted or reflected light, was well-adapted for prismatic analysis.

By raising the eye exactly to the horizontal level of the surface of the liquid, the whole of that surface became of course foreshortened into a narrow blue line. And in this situation it became perfectly evident that this line was not a mere elongated ellipse, the perspective representation of the circular area of the surface, but a very narrow parallelogram, having a breadth of about a fiftieth of an inch, of a vivid and nearly uniform blue colour over its whole breadth. This proves that the epipolic dispersion takes place *within* the liquid, and almost wholly within a distance not exceeding one-fiftieth of an inch from the surface. I say *almost* wholly; for when a sunbeam was directed downwards on the surface, by total reflexion from the base of a prism, a feeble blue gleam was observed to extend downwards below this vivid

line to nearly half an inch from the surface, thus leaving it doubtful whether some small amount of dispersion may not be effected in the interior of the medium at appreciable depths.

The narrow blue line above described was viewed through a Fraunhofer flint prism. The spectrum was deficient at the red end by the totality of the purer and less refrangible red, nearly the whole orange, and all the yellow. A rich and broad band of fine green light slightly fringed with red on the less refrangible side, passed suddenly, on the more refrangible, to a copious indigo and violet without any intermediately graduating blue. Either from want of sufficient brightness, or from some other cause, no black lines were seen; as far as mere illumination went, the spectrum developed appeared continuous.

It appears from this that no one prismatic ray in particular is selected for epipolic dispersion, but that a certain small per-centage of rays extending over a great range of refrangibility are subject to be so affected, the less refrangible extreme being however wholly excluded, as well as the majority of all below a mean refrangibility.

The epipolic colour is more intense the more oblique the visual ray is to the dispersing surface. This, which would be inexplicable on the supposition of the dispersion being effected rigorously at the *geometrical* surface of the medium, is a necessary consequence of its taking place within a superficial stratum of very small, but appreciable thickness, or according to a law of intensity decreasing with great rapidity as the depth within the medium increases. It has been already shown that the dispersion is not confined to the interior of the liquid, but that a large portion of the dispersed light is directed *outwards*, Exp. 4. The more oblique portions of this (which are also the more intense) require, as is there shown, peculiar management to render them visible. Those whose inclination to the dispersive surface is greater, may also be subjected to ocular inspection, by carefully destroying all regularly reflected or accidental light. Thus, if on a surface of black paper two blots be made, the one of water, the other of a solution of quinine, and if these be laid before a window and viewed through a blackened tube in any direction but that of regular reflexion, the water will appear perfectly black, the quinine feebly blue. But however oblique to the surface the visual ray may be in this case, no great accession of intensity takes place in the epipolic tint, for this obvious reason, *that* the dispersing stratum being *within* the medium, no ray dispersed by it can penetrate the surface, which has not an inclination thereto exceeding 41° 22′, at which angle, therefore, it must cut the stratum, and cannot therefore traverse any great extent of it bodily.

Hence, moreover, on the other hand, the internally dispersed light, at great obliquities to the surface (supposed in contact with air), will be reinforced by all that portion which would have penetrated the surface and gone into the air but for the law of total reflexion; all the dispersed rays, that is to say, whose inclination to the surface is less than 41° 22′. This consideration helps to explain the great comparative intensity which the dispersed beam possesses under such circumstances.

As it has been clearly shown that a beam of white light from which certain rays have been separated by epipolic dispersion is no longer susceptible of producing the epipolic phenomena, it would seem a natural and almost a necessary conclusion, that the rays so separated ought to be *wholly*, or in a very high degree, so dispersed when incident on an epipolizing surface. But the whole history of physical optics is one continued warning against such seeming logical conclusions; and in this case also the conclusion is not borne out by fact. Thus in Exp. 2 and 4, abundance of rays internally dispersed must of necessity have been incident on the new surface presented to them, yet no fresh dispersion whatever took place. I may add too that in experiments made with considerable care to exclude all other light from incidence on a quiniferous surface, but such as had originated in epipolic dispersion, I have not succeeded in obtaining any indication of their susceptibility of being a second time so dispersed. Though from the obscurity of such rays as compared with direct light, these trials can hardly be considered as proving a negative, yet they certainly go very far towards proving the absence of any *peculiar* susceptibility in those rays to this particular affection.

J. F. W. HERSCHEL.

Collingwood, March 1, 1845.

Note added during the Printing.—Professor GRAHAM has had the kindness to transmit to me a specimen of an alkaloid, extracted from the brown coat of the seed of the chestnut, to which the name *Esculine* has been given, which possesses in perfection the property of epipolic dispersion when in dilute solution, in which state it precisely resembles quinine. The same eminent chemist refers also to a peculiar oil called Colophene, formed by the regulated action of sulphuric acid on oil of turpentine, which by his description of its phenomena, must also be an epipolizing liquid of a similar character.

J. F. W. H.

May 12, 1845.

VI. *On the Liquefaction and Solidification of Bodies generally existing as Gases.* *By* MICHAEL FARADAY, *Esq., D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, Foreign Associate of the Acad. Sciences, Paris, Corr. Memb. Royal and Imp. Acadd. of Sciences, Petersburgh, Florence, Copenhagen, Berlin, Göttingen, Modena, Stockholm, &c. &c.*

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THE experiments formerly made on the liquefaction of gases*, and the results which from time to time have been added to this branch of knowledge, especially by M. THILORIER†, have left a constant desire on my mind to renew the investigation. This, with considerations arising out of the apparent simplicity and unity of the molecular constitution of all bodies when in the gaseous or vaporous state, which may be expected, according to the indications given by the experiments of M. CAGNIARD DE LA TOUR, to pass by some simple law into their liquid state, and also the hope of seeing nitrogen, oxygen, and hydrogen, either as liquid or solid bodies, and the latter probably as a metal, have lately induced me to make many experiments on the subject; and though my success has not been equal to my desire, still I hope some of the results obtained, and the means of obtaining them, may have an interest for the Royal Society; more especially as the application of the latter may be carried much further than I as yet have had opportunity of applying them. My object, like that of some others, was to subject the gases to considerable pressure with considerable depression of temperature. To obtain the pressure, I used mechanical force, applied by two air-pumps fixed to a table. The first pump had a piston of an inch in diameter, and the second a piston of only half an inch in diameter; and these were so associated by a connecting pipe, that the first pump forced the gas into and through the valves of the second, and then the second could be employed to throw forward this gas, already condensed to ten, fifteen, or twenty atmospheres, into its final recipient at a much higher pressure.

The gases to be experimented with were either prepared and retained in gas holders or gas jars, or else, when the pumps were dispensed with, were evolved in strong glass vessels, and sent under pressure into the condensing tubes. When the gases were over water, or likely to contain water, they passed, in their way from the air-holder to the pump, through a coil of thin glass tube retained in a vessel filled with a good mixture of ice and salt, and therefore at the temperature of 0° FAHR.; the water that was condensed here was all deposited in the first two inches of the coil.

* Philosophical Transactions, 1823, pp. 160, 189. † Annales de Chimie, 1835, lx. 427, 432.

The condensing tubes were of green bottle glass, being from $\frac{1}{6}$th to $\frac{1}{4}$th of an inch external diameter, and from $\frac{1}{42}$d to $\frac{1}{30}$th of an inch in thickness. They were chiefly of two kinds, about eleven and nine inches in length; the one, when horizontal, having a curve downward near one end to dip into a cold bath, and the other, being in form like an inverted siphon, could have the bend cooled also in the same manner when necessary. Into the straight part of the horizontal tube, and the longest leg of the siphon tube, pressure gauges were introduced when required.

Fig. 1.

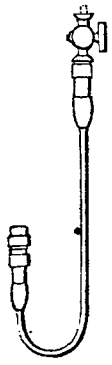

Fig. 2.

Caps, stop-cocks and connecting pieces were employed to attach the glass tubes to the pumps, and these, being of brass, were of the usual character of those employed for operations with gas, except that they were small and carefully made. The caps were of such size that the ends of the glass tubes entered freely into them, and had rings or a female screw worm cut in the interior, against which the cement was to adhere. The ends of the glass tubes were roughened by a file, and when a cap was to be fastened on, both it and the end of the tube were made so warm that the cement*, when applied, was thoroughly melted in contact with these parts, before the tube and cap were brought together and finally adjusted to each other. These junctions bore a pressure of thirty, forty, and fifty atmospheres, with only one failure, in above one hundred instances; and that produced no complete separation of parts, but simply a small leak.

The caps, stop-cocks, and connectors, screwed one into the other, having one common screw thread, so as to be combined in any necessary manner. There were also screw plugs, some solid, with a male screw to close the openings or ends of caps, &c., others with a female screw to cover and close the ends of stop-cocks. All these screw joints were made tight by leaden washers; and by having these of different thickness, equal to from $\frac{4}{8}$th to $\frac{12}{8}$th of the distance between one turn of the screw thread and the next, it was easy at once to select the washer which should allow a sufficient compression in screwing up to make all air-tight, and also bring every part of the apparatus into its right position.

* Five parts of resin, one part of yellow bees'-wax, and one part of red ochre, by weight, melted together.

I have often put a pressure of fifty atmospheres into these tubes, and have had no accident or failure (except the one mentioned). With the assistance of Mr. ADDAMS I have tried their strength by a hydrostatic press, and obtained the following results:—A tube having an external diameter of 0·24 of an inch and a thickness of 0·0175 of an inch, burst with a pressure of sixty-seven atmospheres, reckoning one atmosphere as 15 lb. on the square inch. A tube which had been used, of the shape of fig. 1, its external diameter being 0·225 of an inch, and its thickness about 0·03 of an inch, sustained a pressure of 118 atmospheres without breaking, or any failure of the caps or cement, and was then removed for further use.

A tube such as I have employed for generating gases under pressure, having an external diameter of 0·6 of an inch, and a thickness of 0·035 of an inch, burst at twenty-five atmospheres.

Having these data, it was easy to select tubes abundantly sufficient in strength to sustain any force which was likely to be exerted within them in any given experiment.

The gauge used to estimate the degree of pressure to which the gas within the condensing tube was subjected was of the same kind as those formerly described*, being a small tube of glass closed at one end with a cylinder of mercury moving in it. So the expression of ten or twenty atmospheres, means a force which is able to compress a given portion of air into $\frac{1}{10}$th or $\frac{1}{20}$th of its bulk at the pressure of one atmosphere of thirty inches of mercury. These gauges had their graduation marked on them with a black varnish, and also with Indian ink:—there are several of the gases which, when condensed, cause the varnish to liquefy, but then the Indian ink stood. For further precaution, an exact copy of the gauge was taken on paper, to be applied on the outside of the condensing tube. In most cases, when the experiment was over, the pressure was removed from the interior of the apparatus, to ascertain whether the mercury in the gauge would return back to its first or starting-place.

For the application of cold to these tubes a bath of THILORIER'S mixture of solid carbonic acid and ether was used. An earthenware dish of the capacity of four cubic inches or more was fitted into a similar dish somewhat larger, with three or four folds of dry flannel intervening, and then the bath mixture was made in the inner dish. Such a bath will easily continue for twenty or thirty minutes, retaining solid carbonic acid the whole time; and the glass tubes used would sustain sudden immersion in it without breaking.

But as my hopes of any success beyond that heretofore obtained depended more upon depression of temperature than on the pressure which I could employ in these tubes, I endeavoured to obtain a still greater degree of cold. There are, in fact, some results producible by cold which no pressure may be able to effect. Thus, solidification has not as yet been conferred on a fluid by any degree of pressure. Again, that beautiful condition which CAGNIARD DE LA TOUR has made known, and

* Philosophical Transactions, 1823, p. 192.

which comes on with liquids at a certain heat, may have its point of temperature for some of the bodies to be experimented with, as oxygen, hydrogen, nitrogen, &c., below that belonging to the bath of carbonic acid and ether; and, in that case, no pressure which any apparatus could bear would be able to bring them into the liquid or solid state.

To procure this lower degree of cold, the bath of carbonic acid and ether was put into an air-pump, and the air and gaseous carbonic acid rapidly removed. In this way the temperature fell so low, that the vapour of carbonic acid given off by the bath, instead of having a pressure of one atmosphere, had only a pressure of $\frac{1}{24}$th of an atmosphere, or 1·2 inch of mercury; for the air-pump barometer could be kept at 28·2 inches when the ordinary barometer was at 29·4. At this low temperature the carbonic acid mixed with the ether was not more volatile than water at the temperature of 86°, or alcohol at ordinary temperatures.

In order to obtain some idea of this temperature, I had an alcohol thermometer made, of which the graduation was carried below 32° Fahr., by degrees equal in capacity to those between 32° and 212°. When this thermometer was put into the bath of carbonic acid and ether surrounded by the air, but covered over with paper, it gave the temperature of 106° below 0°. When it was introduced into the bath under the air-pump, it sank to the temperature of 166° below 0°; or 60° below the temperature of the same bath at the pressure of one atmosphere, *i. e.* in the air. In this state the ether was very fluid, and the bath could be kept in good order for a quarter of an hour at a time.

As the exhaustion proceeded I observed the temperature of the bath and the corresponding pressure, at certain other points, of which the following may be recorded:— The external barometer was 29·4 inches:

	inch.		Fahr.
when the mercury in the air-pump barometer was	1	the bath temperature was	−106°,
.. ..	10	.. ..	−112½,
.. ..	20	.. ..	−121,
.. ..	22	.. ..	−125,
.. ..	24	.. ..	−131,
.. ..	26	.. ..	−139,
.. ..	27	.. ..	−146,
.. ..	28	.. ..	−160,
.. ..	28·2	.. ..	−166;

but as the thermometer takes some time to acquire the temperature of the bath, and the latter was continually falling in degree; as also the alcohol thickens considerably at the lower temperature, there is no doubt that the degrees expressed are not so low as they ought to be, perhaps even by 5° or 6° in most cases.

With *dry* carbonic acid under the air-pump receiver I could raise the pump barometer to twenty-nine inches when the external barometer was at thirty inches.

The arrangement by which this cooling power was combined in its effect on gases with the pressure of the pumps, was very simple in principle. An air-pump receiver open at the top was employed; the brass plate which closed the aperture had a small brass tube about six inches long, passing through it air-tight by means of a stuffing-box, so as to move easily up and down in a vertical direction. One of the glass condensing siphon tubes, already described, fig. 1, was screwed on to the lower end of the sliding tube, and the upper end of the latter was connected with a communicating tube in two lengths, reaching from it to the condensing pumps; this tube was small, of brass, and $9\frac{1}{2}$ feet in length; it passed six inches horizontally from the condensing pumps, then rose vertically for two feet, afterwards proceeded horizontally for seven feet, and finally turned down and was immediately connected with the sliding tube. By this means the latter could be raised and lowered vertically, without any strain upon the connexions, and the condensing tube lowered into the cold bath *in vacuo*, or raised to have its contents examined at pleasure. The capacity of the connecting tubes beyond the last condensing pump was only two cubic inches.

When experimenting with any particular gas, the apparatus was put together fast and tight, except the solid terminal screw-plug at the short end of the condensing tube, which being the very extremity of the apparatus, was left a little loose. Then, by the condensing pumps, abundance of gas was passed through the apparatus to sweep out every portion of air, after which the terminal plug was screwed up, the cold bath arranged, and the combined effects of cold and pressure brought to unite upon the gas.

There are many gases which condense at less than the pressure of one atmosphere when submitted to the cold of a carbonic acid bath in air (which latter can upon occasions be brought considerably below $-106°$ Fahr.). These it was easy, therefore, to reduce, by sending them through small conducting tubes into tubular receivers placed in the cold bath. When the receivers had previously been softened in a spirit lamp flame, and narrow necks formed on them, it was not difficult by a little further management, hermetically to seal up these substances in their condensed state. In this manner chlorine, cyanogen, ammonia, sulphuretted hydrogen, arseniuretted hydrogen, hydriodic acid, hydrobromic acid, and even carbonic acid, were obtained, sealed up in tubes in the liquid state; and euchlorine was also secured in a tube receiver with a cap and screw-plug. By using a carbonic acid bath, first cooled *in vacuo*, there is no doubt other condensed gases could be secured in the same way.

The fluid carbonic acid was supplied to me by Mr. Addams, in his perfect apparatus, in portions of about 220 cubic inches each. The solid carbonic acid, when produced from it, was preserved in a glass; itself retained in the middle of three concentric glass jars, separated from each other by dry jackets of woollen cloth. So effectual was this arrangement, that I have frequently worked for a whole day of twelve and fourteen hours, having solid carbonic acid in the reservoir, and enough for all

the baths I required during the whole time, produced by one supply of 220 cubic inches*.

By the apparatus, and in the manner, now described, all the gases before condensed were very easily reduced, and some new results were obtained. When a gas was liquefied, it was easy to close the stop-cock, and then remove the condensing tube with the fluid from the rest of the apparatus. But in order to preserve the liquid from escaping as gas, a further precaution was necessary; namely, to cover over the exposed end of the stop-cock by a blank female screw-cap and leaden washer, and also to tighten perfectly the screw of the stop-cock plug. With these precautions I have kept carbonic acid, nitrous oxide, fluosilicon, &c. for several days.

Even with gases which could be condensed by the carbonic acid bath in air, this apparatus in the air-pump had, in one respect, the advantage; for when the condensing tube was lifted out of the bath into the air, it immediately became covered with hoar frost, obscuring the view of that which was within; but *in vacuo* this was not the case, and the contents of the tube could be very well examined by the eye.

Olefiant gas.—This gas condensed into a clear, colourless, transparent fluid, but did not become solid even in the carbonic acid bath *in vacuo*; whether this was because the temperature was not low enough, or for other reasons referred to in the account of euchlorine, is uncertain.

The pressure of the vapour of this substance at the temperature of the carbonic acid bath in air (−103° FAHR.) appeared singularly uncertain, being on different occasions, and with different specimens, 3·7, 8·7, 5 and 6 atmospheres. The Table below shows the tension of vapour for certain degrees below 0° FAHR., with two different specimens obtained at different times, and it will illustrate this point.

FAHR.	Atmospheres.	Atmospheres.
−100°	4·60	9·30
− 90	5·68	10·26
− 80	6·92	11·33
− 70	8·32	12·52
− 60	9·88	13·86
− 50	11·72	15·36
− 40	13·94	17·05
− 30	16·56	18·98
− 20	19·58	21·23
− 10		23·89
0		27·18
10		31·70
20		36·80
30		42·50

* On one occasion the solid carbonic acid was exceedingly electric, but I could not produce the effect again: it was probably connected with the presence of oil which was in the carbonic acid box; neither it nor the fila-

I have not yet resolved this irregularity, but believe there are two or more substances, physically, and perhaps occasionally chemically different, in olefiant gas; and varying in proportion with the circumstances of heat, proportions of ingredients, &c. attending the preparation.

The fluid affected the resin of the gauge graduation, and probably also the resin of the cap cement, though slowly.

Hydriodic acid.—This substance was prepared from the iodide of phosphorus by heating it with a very little water. It is easily condensable by the temperature of a carbonic acid bath: it was redistilled, and thus obtained perfectly pure.

The acid may be obtained either in the solid or liquid, or (of course) in the gaseous state. As a solid it is perfectly clear, transparent, and colourless; having fissures or cracks in it resembling those that run through ice. Its solidifying temperature is nearly −60° FAHR., and then its vapour has not the pressure of one atmosphere; at a point a little higher it becomes a clear liquid, and this point is close upon that which corresponds to a vaporous pressure of one atmosphere. The acid dissolves the cap cement and the bitumen of the gauge graduation; and appears also to dissolve and act on fat, for it leaked by the plug of the stop-cock with remarkable facility. It acts on the brass of the apparatus, and also on the mercury in the gauge. Hence the following results as to pressures and temperatures are not to be considered more than approximations:—

At 0° FAHR. pressure was 2·9 atmospheres.
At 32° FAHR. pressure was 3·97 atmospheres.
At 60° FAHR. pressure was 5·86 atmospheres.

Hydrobromic acid.—This acid was prepared by adding to perbromide of phosphorus* about one-third of its bulk of water in a proper distillatory apparatus formed of glass tube, and then applying heat to distil off the gaseous acid. This being sent into a very cold receiver, was condensed into a liquid, which being rectified by a second distillation, was then experimented with.

Hydrobromic acid condenses into a clear colourless liquid at 100° below 0°, or lower, and has not the pressure of one atmosphere at the temperature of the carbonic acid bath in air. It soon obstructs and renders the motion of the mercury in the air-gauge irregular, so that I did not obtain a measure of its elastic force; but it is less than that of muriatic acid. At and below the temperature of −124° FAHR. it is a

ments of ice which formed on it in the air conducted, for when touched it preserved its electric state. Believing as yet that the account I have given of the cause of the electric state of an issuing jet of steam and water (Phil. Trans. 1843, p. 17) is the true one, I conclude that this also was a case of the production of electricity simply by friction, and unconnected with vaporization.

* The bromides of phosphorus are easily made without risk of explosion. If a glass tube be bent so as to have two depressions, phosphorus placed in one and bromine in the other; then by inclining the tube, the vapour of bromine can be made to flow gradually on to, and combine with, the phosphorus. The fluid protobromide is first formed, and this is afterwards converted into solid perbromide. The excess of bromine may be dissipated by the careful application of heat.

solid, transparent, crystalline body. It does not freeze until reduced much lower than this temperature; but being frozen by the carbonic acid bath *in vacuo*, it remains a solid until the temperature in rising attains to —124°.

Fluosilicon.—I found that this substance in the gaseous state might be brought in contact with the oil and metal of the pumps, without causing injury to them, for a time sufficiently long to apply the joint process of condensation already described. The substance liquefied under a pressure of about nine atmospheres at the lowest temperature, or at 160° below 0°; and was then clear, transparent, colourless, and very fluid like hot ether. It did not solidify at any temperature to which I could submit it. I was able to preserve it in the tube until the next day. Some leakage had then taken place (for it ultimately acted on the lubricating fat of the stop-cock), and there was no liquid in the tube at common temperatures; but when the bend of the tube was cooled to 32° by a little ice, fluid appeared: a bath of ice and salt caused a still more abundant condensation. The pressure appeared then to be above thirty atmospheres, but the motion of the mercury in the gauge had become obstructed through the action of the fluosilicon, and no confidence could be reposed in its indications.

Phosphuretted hydrogen.—This gas was prepared by boiling phosphorus in a strong pure solution of caustic potassa, and the gas was preserved over water in a dark room for several days to cause the deposition of any mere vapour of phosphorus which it might contain. It was then subjected to high pressure in a tube cooled by a carbonic acid bath, which had itself been cooled under the receiver of the air-pump. The gas in its way to the pumps passed through a long spiral of thin narrow glass tube immersed in a mixture of ice and salt at 0°, to remove as much water from it as possible.

By these means the phosphuretted hydrogen was liquefied; for a pure, clear, colourless, transparent and very limpid fluid appeared, which could not be solidified by any temperature applied, and which when the pressure was taken off immediately rose again in the form of gas. Still the whole of the gas was not condensable into this fluid. By working the pumps the pressure would rise up to twenty-five atmospheres at this very low temperature, and yet at the pressure of two or three atmospheres and the same temperature, liquid would remain. There can be no doubt that phosphuretted hydrogen condensed, but neither can there be a doubt that some other gas, not so condensable, was also present, which perhaps may be either another phosphuretted hydrogen or hydrogen itself.

Fluoboron.—This substance was prepared from fluor spar, fused boracic acid and strong sulphuric acid, in a tube generator such as that already described, and conducted into a condensing tube under the generating pressure. The ordinary carbonic acid bath did not condense it, but the application of one cooled under the air-pump caused its liquefaction, and fluoboron then appeared as a very limpid, colourless, clear fluid, showing no signs of solidification, but when at the lowest temperature mobile as hot ether. When the pressure was taken off, or the temperature raised, it returned into the state of gas.

The following are some results of pressure, all that I could obtain with the liquid in my possession; for, as the liquid is light and the gas heavy, the former rapidly disappears in producing the latter. They make no pretensions to accuracy, and are given only for general information.

Fahr.	Atmospheres.	Fahr.	Atmospheres.	Fahr.	Atmospheres.
—100°	4·61	—72°	9·23	—62°	11·54.
— 82	7·5	—66	10·00		

The preceding are, as far as I am aware, new results of the liquefaction and solidification of gases. I will now briefly add such other information respecting solidification, pressure, &c., as I have obtained with gaseous bodies previously condensed. As to pressure, considerable irregularity often occurred, which I cannot always refer to its true cause; sometimes a little of the compressed gas would creep by the mercury in the gauge, and increase the volume of inclosed air; and this varied with different substances, probably by some tendency which the glass had to favour the condensation of one (by something analogous to hygrometric action) more than another. But even when the mercury returned to its place in the gauge, there were anomalies which seemed to imply, that a substance, supposed to be one, might be a mixture of two or more. It is, of course, essential that the gauge be preserved at the same temperature throughout the observations.

Muriatic acid.—This substance did not freeze at the lowest temperature to which I could attain. Liquid muriatic acid dissolves bitumen; the solution, liberated from pressure, boils, giving off muriatic acid vapour, and the bitumen is left in a solid frothy state, and probably altered, in some degree, chemically. The acid unites with and softens the resinous cap cement, but leaves it when the pressure is diminished. The following are certain pressures and temperatures which, I believe, are not very far from truth; the marked numbers are from experiment.

Fahr.	Atmospheres.	Fahr.	Atmospheres.	Fahr.	Atmospheres.
∪ —100°	1·80	∪ —53°	5·83	∪ — 5°	13·88
∪ — 92	2·28	—50	6·30	∪ 0	15·04
— 90	2·38	∪ —42	7·40	10	17·74
∪ — 83	2·90	—40	7·68	20	21·09
— 80	3·12	∪ —33	8·53	∪ 25	23·08
∪ — 77	3·37	—30	9·22	30	25·32
— 70	4·02	∪ —22	10·66	∪ 32	26·20
∪ — 67	4·26	—20	10·92	40	30·67
— 60	5·08	—10	12·82		

The result formerly obtained* was forty atmospheres at the temperature of 50° Fahr.

* Philosophical Transactions, 1823, p. 198.

Sulphurous acid.—When liquid, it dissolves bitumen. It becomes a crystalline, transparent, colourless, solid body, at —105° FAHR.; when partly frozen the crystals are well-formed. The solid sulphurous acid is heavier than the liquid, and sinks freely in it. The following is a table of pressures in atmospheres of 30 inches mercury, of which the marked results are from many observations, the others are interpolated. They differ considerably from the results obtained by BUNSEN*, but agree with my first and only result.

FAHR.	Atmospheres.	FAHR.	Atmospheres.	FAHR.	Atmospheres.
0	0·725	40	1·78	76·8	3·50
10	0·92	46·5	2·00	85	4·00
⌣14	1·00	⌣48	2·06	⌣ 90	4·35
⌣19	1·12	⌣56	2·42	93	4·50
⌣23	1·23	58	2·50	98	5·00
⌣26	1·33	⌣64	2·76	⌣100	5·16
31·5	1·50	68	3·00	104	5·50
⌣32	1·53	⌣73·5	3·28	110	6·00
⌣33	1·57				

Sulphuretted hydrogen.—This substance solidifies at 122° FAHR. below 0°, and is then a white crystalline translucent substance, not remaining clear and transparent in the solid state like water, carbonic acid, nitrous oxide, &c., but forming a mass of confused crystals like common salt or nitrate of ammonia, solidified from the melted state. As it fuses at temperatures above —122°, the solid part sinks freely in the fluid, indicating that it is considerably heavier. At this temperature the pressure of its vapour is less than one atmosphere, not more, probably, than 0·8 of an atmosphere, so that the liquid allowed to evaporate in the air would not solidify as carbonic acid does.

The following is a table of the tension of its vapour, the marked numbers being close to experimental results, and the rest interpolated. The curve resulting from these numbers, though coming out nearly identical in different series of experiments, is apparently so different in its character from that of water or carbonic acid, as to leave doubts on my mind respecting it, or else of the identity of every portion of the fluid obtained, yet the crystallization and other characters of the latter seemed to show that it was a pure substance.

* Bibliothèque Universelle, 1839, xxiii. p. 185.

Fahr.	Atmospheres.	Fahr.	Atmospheres.	Fahr.	Atmospheres.
−100°	1·02	−50°	2·35	0°	6·10
∪— 94	1·09	∪—45	2·59	10	7·21
∪— 90	1·15	∪—40	2·86	20	8·44
∪— 83	1·27	−30	3·49	∪26	9·36
— 80	1·33	∪—24	3·95	30	9·94
∪— 74	1·50	∪—20	4·24	40	11·84
— 70	1·59	∪—16	4·60	∪48	13·70
∪— 68	1·67	−10	5·11	50	14·14
— 60	1·93	∪— 2	5·90	∪52	14·60
∪— 58	2·00				

Carbonic acid.—The solidification of carbonic acid by M. Thilorier is one of the most beautiful experimental results of modern times. He obtained the substance, as is well known, in the form of a concrete white mass like fine snow, aggregated. When it is melted and resolidified by a bath of low temperature, it then appears as a clear, transparent, crystalline, colourless body, like ice; so clear, indeed, that at times it was doubtful to the eye whether anything was in the tube, yet at the same time the part was filled with solid carbonic acid. It melts at the temperature of −70° or −72° Fahr., and the solid carbonic acid is heavier than the fluid bathing it. The solid or liquid carbonic acid at this temperature has a pressure of 5·33 atmospheres nearly. Hence it is easy to understand the readiness with which liquid carbonic acid, when allowed to escape into the air, exerting only a pressure of one atmosphere, freezes a part of itself by the evaporation of another part.

Thilorier gives −100° C. or −148° Fahr. as the temperature at which carbonic acid becomes solid. This however is rather the temperature to which solid carbonic acid can sink by further evaporation in the air, and is a temperature belonging to a pressure, not only lower than that of 5·33 atmospheres, but even much below that of one atmosphere. This cooling effect to temperatures below the boiling-point often appears. A bath of carbonic acid and ether exposed to the air will cool a tube containing condensed solid carbonic acid, until the pressure within the tube is less than one atmosphere; yet, if the same bath be covered up so as to have the pressure of one atmosphere of carbonic acid vapour over it, then the temperature is such as to produce a pressure of 2·5 atmospheres by the vapour of the solid carbonic acid within the tube.

The estimates of the pressure of carbonic acid vapour are sadly at variance; thus, Thilorier* says it has a pressure of 26 atmospheres at −4° Fahr., whilst Addams† says that for that pressure it requires a temperature of 30°. Addams gives the pressure about 27½ atmospheres at 32°, but Thilorier and myself‡ give it as 36 atmospheres at the same temperature. At 50° Brunel§ estimates the pressure as 60

* Annales de Chimie, 1835, lx. 427, 432.

† Report of British Association, 1838, p. 70.

‡ Philosophical Transactions, 1823, p. 193.

§ Royal Institution Journal, xxi. 132.

atmospheres, whilst ADDAMS makes it only 34·67 atmospheres. At 86° THILORIER finds the pressure to be 73 atmospheres; at 4° more, or 90°, BRUNEL makes it 120 atmospheres; and at 10° more, or 100°, ADDAMS makes it less than THILORIER at 86°, and only 62·32 atmospheres; even at 150° the pressure with him is not quite 100 atmospheres.

I am inclined to think that at about 90° CAGNIARD DE LA TOUR'S state comes on with carbonic acid. From THILORIER'S data we may obtain the specific gravity of the liquid and the vapour over it at the temperature of 86° FAHR., and the former is little more than twice that of the latter; hence a few degrees more of temperature would bring them together, and BRUNEL'S result seems to imply that the state was then on, but in that case ADDAMS'S results could only be accounted for by supposing that there was a deficiency of carbonic acid. The following are the pressures which I have recently obtained:—

FAHR.	Atmospheres.	FAHR.	Atmospheres.	FAHR.	Atmospheres.
∪ −111°	1·14	−60°	6·97	∪ − 4°	21·48
−110	1·17	∪ −56	7·70	0	22·84
∪ −107	1·36	−50	8·88	∪ 5	24·75
−100	1·85	−40	11·07	∪ 10	26·82
∪ − 95	2·28	∪ −34	12·50	∪ 15	29·09
− 90	2·77	−30	13·54	20	30·65
∪ − 83	3·60	∪ −23	15·45	∪ 23	33·15
− 80	3·93	−20	16·30	30	37·19
∪ − 75	4·60	∪ −15	17·80	∪ 32	38·50
− 70	5·33	−10	19·38		

Carbonic acid is remarkable amongst bodies for the high tension of the vapour which it gives off whilst in the solid or glacial state. There is no other substance which at all comes near it in this respect, and it causes an inversion of what in all other cases is the natural order of events. Thus, if, as is the case with water, ether, mercury or any other fluid, that temperature at which carbonic acid gives off vapour equal in elastic force to one atmosphere, be called its boiling-point; or, if (to produce the actual effect of ebullition) the carbonic acid be plunged below the surface of alcohol or ether, then we shall perceive that the freezing and boiling-points are inverted, i. e. that the freezing-point is the hotter, and the boiling-point the colder of the two, the latter being about 50° below the former.

Euchlorine.—This substance was easily converted from the gaseous state into a solid crystalline body, which, by a little increase of temperature, melted into an orange-red fluid, and by diminution of temperature again congealed; the solid euchlorine had the colour and general appearance of bichromate of potassa; it was moderately hard, brittle and translucent; and the crystals were perfectly clear. It melted at the temperature of 75° below 0°, and the solid portion was heavier than the liquid.

When in the solid state it gives off so little vapour that the eye is not sensible of its presence by any degree of colour in the air over it when looking down a tube four inches in length, at the bottom of which is the substance. Hence the pressure of its vapour at that temperature must be very small.

Some hours after, wishing to solidify the same portion of euchlorine which was then in a liquid state, I placed the tube in a bath at $-110°$, but could not succeed either by continuance of the tube in the bath, or shaking the fluid in the tube, or opening the tube to allow the full pressure of the atmosphere; but when the liquid euchlorine was touched by a platinum wire it instantly became solid, and exhibited all the properties before described. There are many similar instances amongst ordinary substances, but the effect in this case makes me hesitate in concluding that all the gases which as yet have refused to solidify at temperatures as low as 166° below 0°, cannot acquire the solid state at such a temperature.

Nitrous oxide.—This substance was obtained *solid* by the temperature of the carbonic acid bath *in vacuo*, and appeared as a beautiful clear crystalline colourless body. The temperature required for this effect must have been very nearly the lowest, perhaps about 150° below 0°. The pressure of the vapour rising from the solid nitrous oxide was less than one atmosphere.

Hence it was concluded that liquid nitrous oxide could not freeze itself by evaporation at one atmosphere, as carbonic acid does; and this was found to be true, for when a tube containing much liquid was freely opened, so as to allow evaporation down to one atmosphere, the liquid boiled and cooled itself, but remained a liquid. The cold produced by the evaporation was very great, and this was shown by putting the part of the tube containing the liquid nitrous oxide, into a cold bath of carbonic acid, for the latter was like a hot bath to the former, and instantly made it boil rapidly.

I kept this substance for some weeks in a tube closed by stop-cocks and cemented caps. In that time there was no action on the bitumen of the graduation, nor on the cement of the caps; these bodies remained perfectly unaltered.

Hence it is probable that this substance may be used in certain cases, instead of carbonic acid, to produce degrees of cold far below those which the latter body can supply. Down to a certain temperature, that of its solidification, it would not even require ether to give contact, and below that temperature it could easily be used mingled with ether; its vapour would do no harm to an air-pump, and there is no doubt that the substance placed *in vacuo* would acquire a temperature lower than any as yet known, perhaps as far below the carbonic acid bath *in vacuo* as that is below the same bath in air.

This substance, like olefiant gas, gave very uncertain results at different times as to the pressure of its vapour; results which can only be accounted for by supposing that there are two different bodies present, soluble in each other, but differing in the elasticity of their vapour. Four different portions gave at the same temperature,

namely, −106° FAHR., the following great differences in pressure, 1·66; 4·4; 5·0; and 6·3 atmospheres, and this after the elastic atmosphere left in the tubes at the conclusion of the condensation had been allowed to escape, and be replaced by a portion of the respective liquids which then rose in vapour. The following Table gives certain results with a portion of liquid which exerted a pressure of six atmospheres at −106° FAHR.

FAHR.	Atmospheres.	Atmospheres.
−40°	10·20	
−35	10·95	
−30	11·80	
−25	12·75	
−20	13·80	
−15	14·95	
−10	16·20	
− 5	17·55	
0	19·05	24·40
5	20·70	26·08
10	22·50	27·84
15	24·45	29·68
20	26·55	31·62
25	28·85	33·66
30		35·82
35		38·10

The second column expresses the pressures given as the fluid was raised from low to higher temperatures. The third column shows the pressures given the next day with the same tube after it had attained to and continued at the atmospheric temperature for some hours. There is a difference of four or five atmospheres between the two, showing that in the first instance the previous low temperature had caused the solution of a more volatile part in the less volatile and liquid portion, and that the prolonged application of a higher temperature during the night had gradually raised it again in vapour. This result occurred again and again with the same specimen*.

Cyanogen.—This substance becomes a solid transparent crystalline body, as BUNSEN has already stated†, which raised to the temperature of −30° FAHR. then liquefies. The solid and liquid appear to be nearly of the same specific gravity, but the solid is perhaps the denser of the two.

* This substance is one of those which I liquefied in 1823 (see Philosophical Transactions). Since writing the above I perceive that M. NATTERER has condensed it into the liquid state by the use of pumps only (see Comptes Rendus, 1844, 18th Nov. p. 1111), and obtained the liquid in considerable quantities. The non-solidification of it by exposure to the air perfectly accords with my own results.

† Bibliothèque Universelle, 1839, xxiii. p. 184.

The mixed solid and liquid substance yields a vapour of rather less pressure than one atmosphere. In accordance with this result, if the liquid be exposed to the air, it does not freeze itself as carbonic acid does.

The liquid tends to distil over and condense on the cap cement and bitumen of the gauge, but only slightly. When cyanogen is made from cyanide of mercury sealed up hermetically in a glass tube, the cyanogen distils back and condenses in the paracyanic residue of the distillation, but the pressure of the vapour at common temperatures is still as great, or very nearly so, as if the cyanogen were in a clean separate liquid state.

A measured portion of liquid cyanogen was allowed to escape and expand into gas. In this way one volume of liquid at the temperature of 63° FAHR. gave 393·9 volumes of gas at the same temperature and the barometric pressure of 30·2 inches. If 100 cubic inches of the gas be admitted to weigh 55·5 grains, then a cubic inch of the liquid would weigh 218·6 grains. This gives its specific gravity as 0·866. When first condensed I estimated it as nearly 0·9.

Cyanogen is a substance which yielded on different occasions results of vaporous tension differing much from each other, though the substance appeared always to be pure. The following are numbers in which I place some confidence, the pressures being in atmospheres of 30 inches of mercury, and the marked results experimental*.

FAHR.	Atmospheres.	FAHR.	Atmospheres.	FAHR.	Atmospheres.
0°	1·25	∪38·5	2·72	77°	5·00
8·5	1·5	∪44·5	3·00	∪79	5·16
∪10	1·53	∪48	3·17	83	5·50
15	1·72	∪50	3·28	88·3	6·00
∪20	1·89	∪52	3·36	∪93·5	6·50
22·8	2·00	54·3	3·50	∪95	6·64
∪27	2·20	∪63	4·00	98·4	7·00
∪32	2·37	∪70	4·50	∪103	7·50
34·5	2·50	∪74	4·79		

Ammonia.—This body may be obtained as a *solid*, white, translucent, crystalline substance, melting at the temperature of 103° below 0°; at which point the solid substance is heavier than the liquid. In that state the pressure of its vapour must be very small.

Liquid ammonia at 60° was allowed to expand into ammoniacal gas at the same temperature; one volume of the liquid gave 1009·8 volumes of the gas, the barometer being at the pressure of 30·2 inches. If 100 cubic inches of ammoniacal gas be allowed to weigh 18·28 grains, it will give 184·6 grains as the weight of a cubic inch of liquid ammonia at 60°. Hence its specific gravity at that temperature will be 0·731. In the old experiments I found by another kind of process that its specific gravity was 0·76 at 50°.

* See BUNSEN's results, Bibliothèque Universelle, 1839, xxiii. p. 185.

The following is a table of the pressure of ammonia vapour, the marked results, as before, being those obtained by experiment:—

Fahr.	Atmospheres.	Fahr.	Atmospheres.	Fahr.	Atmospheres.
∨0°	2·48	∨41°	5·10	∨61·3°	7·00
0·5	2·50	∨44	5·36	∨65·6	7·50
∨9·3	3·00	∨45	5·45	∨67	7·63
∨18	3·50	45·8	5·50	69·4	8·00
∨21	3·72	∨49	5·83	73	8·50
25·8	4·00	∨51·4	6·00	76·8	9·00
∨26	4·04	∨52	6·10	80	9·50
∨32	4·44	∨55	6·38	∨83	10·00
∨33	4·50	∨56·5	6·50	85	10·30
39·5	5·00	∨60	6·90		

Arseniuretted Hydrogen.—This body, liquefied by Dumas and Soubeiran, did not solidify at the lowest temperature to which I could submit it, i. e. not at 166° below 0° Fahr. In the following table of the elasticity of its vapour the marked results are experimental, and the others interpolated:—

Fahr.	Atmospheres.	Fahr.	Atmospheres.	Fahr.	Atmospheres.
∨−75°	0·94	−30°	2·84	∨10°	6·24
−70	1·08	∨−23	3·32	∨20	7·39
∨−64	1·26	−20	3·51	30	8·66
−60	1·40	−10	4·30	∨32	8·95
∨−52	1·73	∨− 5	4·74	∨40	10·05
−50	1·80	∨ 0	5·21	∨50	11·56
−40	2·28	∨ 3	5·56	∨60	13·19
∨−36	2·50				

The following bodies would not freeze at the very low temperature of the carbonic acid bath *in vacuo* (−166° Fahr.):—Chlorine, ether, alcohol, sulphuret of carbon, caoutchoucine, camphine or rectified oil of turpentine. The alcohol, caoutchoucine, and camphine lost fluidity and thickened somewhat at −106°, and still more at the lower temperature of −166°. The alcohol then poured from side to side like an oil.

Dry yellow fluid nitrous acid when cooled below 0° loses the greater part of its colour, and then fuses into a white, crystalline, brittle and but slightly translucent substance, which fuses a little above 0° Fahr. The green and probably hydrated acid required a much lower temperature for its solidification, and then became a pale bluish solid. There were then evidently two bodies, the dry acid which froze out first, and then the hydrate, which requires at least −30° below 0° before it will solidify.

The following gases showed no signs of liquefaction when cooled by the carbonic acid bath *in vacuo*, even at the pressures expressed :—

	Atmospheres.
Hydrogen at	27
Oxygen at	27
Nitrogen at	50
Nitric oxide at	50
Carbonic oxide at	40
Coal gas	32

The difference in the facility of leakage was one reason of the difference in the pressure applied. I found it impossible, from this cause, to raise the pressure of hydrogen higher than twenty-seven atmospheres by an apparatus that was quite tight enough to confine nitrogen up to double that pressure.

M. Cagniard de la Tour has shown that at a certain temperature, a liquid, under sufficient pressure, becomes clear transparent vapour or gas, having the same bulk as the liquid. At this temperature, or one a little higher, it is not likely that any increase of pressure, except perhaps one exceedingly great, would convert the gas into a liquid. Now the temperature of 166° below 0°, low as it is, is probably *above* this point of temperature for hydrogen, and perhaps for nitrogen and oxygen, and then no compression without the conjoint application of a degree of cold below that we have as yet obtained, can be expected to take from them their gaseous state. Further, as ether assumes this state before the pressure of its vapour has acquired thirty-eight atmospheres, it is more than probable that gases which can resist the pressure of from twenty-seven to fifty atmospheres at a temperature of 166° below 0° could never appear as liquids, or be made to lose their gaseous state at common temperatures. They may probably be brought into the state of very condensed gases, but not liquefied.

Some very interesting experiments on the compression of gases have been made by M. G. Aime*, in which oxygen, olefiant, nitric oxide, carbonic oxide, fluosilicon, hydrogen, and nitrogen gases were submitted to pressures, rising up to 220 atmospheres in the case of the two last; but this was in the depths of the sea where the results under pressure could not be examined. Several of them were diminished in bulk in a ratio far greater than the pressure put upon them; but both M. Cagniard de la Tour and M. Thilorier have shown that this is often the case whilst the substance retains the gaseous form. It is possible that olefiant gas and fluosilicon may have liquefied down below, but they have not yet been seen in the liquid state except in my own experiments, and in them not at temperatures above 40° Fahr. The results with oxygen are so unsteady and contradictory as to cause doubt in regard to those obtained with the other gases by the same process.

Thus, though as yet I have not condensed oxygen, hydrogen, or nitrogen, the ori-

* Annales de Chimie, 1843, viii. 275.

ginal objects of my pursuit, I have added six substances, usually gaseous, to the list of those that could previously be shown in the liquid state, and have reduced seven, including ammonia, nitrous oxide, and sulphuretted hydrogen, into the solid form. And though the numbers expressing tension of vapour cannot (because of the difficulties respecting the use of thermometers and the apparatus generally) be considered as exact, I am in hopes they will assist in developing some general law governing the vaporization of all bodies, and also in illustrating the physical state of gaseous bodies as they are presented to us under ordinary temperature and pressure.

Royal Institution,
Nov. 15, 1844.

NOTE.—*Additional remarks respecting the Condensation of Gases.*

By MICHAEL FARADAY, *Esq.*

Received February 20,—Read February 20, 1845.

Nitrous oxide.—Suspecting the presence on former occasions of nitrogen in the nitrous oxide, and mainly because of muriate in the nitrate of ammonia used, I prepared that salt in a pure state from nitric acid and carbonate of ammonia previously proved, by nitrate of silver, to be free from muriatic acid. After the nitrous oxide prepared from this salt had remained for some days in well-closed bottles in contact with a little water, I condensed it in the manner already described, and when condensed I allowed half the fluid to escape in vapour, that as much as possible of the less condensable portion might be carried off. In this way as much gas as would fill the capacity of the vessels twenty or thirty times or more was allowed to escape. Afterwards the following series of pressures was obtained:—

Fahr.	Atmospheres.	Fahr.	Atmospheres.	Fahr.	Atmospheres.
−125°	1·00	−70°	4·11	−15°	14·69
−120	1·10	−65	4·70	−10	16·15
−115	1·22	−60	5·36	− 5	17·70
−110	1·37	−55	6·09	0	19·34
−105	1·55	−50	6·89	5	21·07
−100	1·77	−45	7·76	10	22·89
− 95	2·03	−40	8·71	15	24·80
− 90	2·34	−35	9·74	20	26·80
− 85	2·70	−30	10·85	25	28·90
− 80	3·11	−25	12·04	30	31·10
− 75	3·58	−20	13·32	35	33·40

These numbers may all be taken as the results of experiments. Where the temperatures are not those actually observed, they are in almost all cases within a degree of it, and proportionate to the effects really observed. The departure of the real observations from the numbers given is very small. This table I consider as far more worthy of confidence than the former, and yet it is manifest that the curve is not consistent with the idea of a pure single substance, for the pressures at the lowest temperature are too high. I believe that there are still two bodies present, and that the more volatile, as before said, is condensable in the liquid of the less volatile; but I think there is a far smaller proportion of the more volatile (nitrogen, or whatever it may be) than in the former case.

Olefiant gas.—The olefiant gas condensed in the former experiment was prepared in the ordinary way, using excellent alcohol and sulphuric acid; then washed by agitation with about half its bulk of water, and finally left for three days over a thick mixture of lime and water with occasional agitation. In this way all the sulphurous and carbonic acids were removed, and I believe all the ether, except such minute portions as could not interfere with my results. In respect of the ether, I have since found that the process is satisfactory; for when I purposely added ether vapour to air, so as to increase its bulk by one-third, treatment like that above removed it, so as to leave the air of its original volume. There was yet a slight odour of ether left, but not so much as that conferred by adding one volume of the vapour of ether to 1200 or 1500 volumes of air. I find that when air is expanded $\frac{1}{4}$th or $\frac{1}{3}$rd more by the addition of the vapour of ether, washing first of all with about $\frac{1}{50}$th of its volume of water, then again with about as much water, and lastly with its volume of water, removes the ether to such a degree, that though a little smell may remain, the air is of its original volume.

As already stated, it is the presence of other and more volatile hydrocarbons than olefiant gas, which the tensions obtained seemed to indicate, both in the gas and the liquid resulting from its condensation. In a further search after these I discovered a property of olefiant gas which I am not aware is known (since I do not find it referred to in books), namely its ready solubility in strong alcohol, ether, oil of turpentine, and such like bodies*. Alcohol will take up two volumes of this gas; ether can absorb two volumes; oil of turpentine two volumes and a half; and olive oil one volume by agitation at common temperatures and pressure; consequently, when a vessel of olefiant gas is transferred to a bath of any of these liquids and agitated, absorption quickly takes place.

Examined in this way, I have found no specimen of olefiant gas that is entirely absorbed; a residue always remains, which, though I have not yet had time to examine it accurately, appears to be light carburetted hydrogen; and I have no doubt that this is the substance which has mainly interfered in my former results. This sub-

* Water, as Berzelius and others have pointed out, dissolves about $\frac{1}{8}$th its volume of olefiant gas, but I find that it also leaves an insoluble residue, which burns like light carburetted hydrogen.

stance appears to be produced in every stage of the preparation of olefiant gas. On taking six different portions of gas at different equal intervals, from first to last, during one process of preparation, after removing the sulphurous and carbonic acid and the ether as before described, then the following was the proportion per cent. of insoluble gas in the remainder when agitated with oil of turpentine, 10·5; 10; 10·1; 13·1; 28·3; 61·8. Whether carbonic oxide was present in any of these undissolved portions I cannot at present say.

In reference to the part dissolved, I wish as yet to guard myself from being supposed to assume that it is one uniform substance; there is indeed little doubt that the contrary is true; for whilst a volume of oil of turpentine introduced into twenty times its volume of olefiant gas cleared from ether and the acids, absorbs $2\frac{1}{2}$ volumes of the gas, the same volume of fresh oil of turpentine brought into similar contact with abundance of the gas which remains when one-half has been removed by solution only dissolved 1·54 part, yet there was an abundant surplus of gas which would dissolve in fresh oil of turpentine at this latter rate. When two-thirds of a portion of fresh olefiant gas were removed by solution, the most soluble portion of that which remained required its bulk of fresh oil of turpentine to dissolve it. Hence at first one volume of camphine dissolved 2·50, but when the richer portion of the gas was removed, one volume dissolved 1·54 part; and when still more of the gas was taken away by solution, one volume of camphine dissolved only one volume of the gas. This can only be accounted for by the presence of various compounds in the soluble portion of the gas.

A portion of good olefiant gas was prepared, well-agitated with its bulk of water in close vessels, left over lime and water for three days, and then condensed as before. When much liquid was condensed, a considerable proportion was allowed to escape to sweep out the uncondensed atmosphere and the more condensable vapours; and then the following pressures were observed:—

Fahr.	Atmospheres.	Fahr.	Atmospheres.	Fahr.	Atmospheres.
−105°	4·60	−65°	8·30	−30°	16·22
−100	4·82	−60	9·14	−25	17·75
− 95	5·10	−55	10·07	−20	19·38
− 90	5·44	−50	11·10	−15	21·11
− 85	5·84	−45	12·23	−10	22·94
− 80	6·32	−40	13·46	− 5	24·87
− 75	6·89	−35	14·79	0	26·90
− 70	7·55				

On examining the form of the curve given by these pressures, it is very evident that, as on former occasions, the pressures at low temperatures are too great to allow the condensed liquid to be considered as one uniform body, and the form of the curve at the higher pressures is quite enough to prove that no ether was present either in

this or the former fluids. On permitting the liquid in the tube to expand into gas, and treating 100 parts of that gas with oil of turpentine, eighty-nine parts were dissolved, and eleven parts remained insoluble. There can be no doubt that the presence of this latter substance, soluble as it is under pressure in the more condensable portions, is the cause of the irregularity of the curve, and the too high pressure at the lower temperatures.

The ethereal solution of olefiant gas being mixed with eight or nine times its volume of water, dissolved and gradually minute bubbles of gas appeared, the separation of which was hastened by a little heat. In this way about half the gas dissolved was re-obtained, and burnt like very rich olefiant gas. One volume of the alcoholic solution, with two volumes of water, gave very little appearance of separating gas. Even the application of heat did not at first cause the separation, but gradually about half the dissolved olefiant gas was liberated.

The separation of the dissolved gas by water, heat, or change of pressure from its solutions, will evidently supply means of procuring olefiant gas in a greater state of purity than heretofore; the power of forming these solutions will also very much assist in the correct analysis of mixtures of hydrocarbons. I find that light carburetted hydrogen is hardly sensibly soluble in alcohol or ether, and in oil of turpentine the proportion dissolved is not probably $\frac{1}{15}$th the volume of the fluid employed; but the further development of these points I must leave for the present.

Carbonic acid.—This liquid may be retained in glass tubes furnished with cemented caps, and closed by plugs or stop-cocks, as described, but it is important to remember the softening action on the cement which, being continued, at last reduces its strength below the necessary point. A tube of this kind was arranged on the 10th of January and left; on the 15th of February it exploded, not by any fracture of the tube, for that remained unbroken, but simply by throwing off the cap through a failure of the cement. Hence the cement joints should not be used for long experiments, but only for those enduring for a few days.

Oxygen.—Chlorate of potassa was melted and pulverized. Oxide of manganese was pulverized, heated red-hot for half an hour, mixed whilst hot with the chlorate, and the mixture put into a long strong glass generating tube with a cap cemented on, and this tube then attached to another with a gauge for condensation. The heat of a spirit lamp carefully applied produced the evolution of oxygen without any appearance of water, and the tubes, both hot and cold, sustained the force generated. In this manner the pressure of oxygen within the apparatus was raised as high as 58·5 atmospheres, whilst the temperature at the condensing place was reduced as low as $-140°$ Fahr., but no condensation appeared. A little above this pressure the cement of two of the caps began to leak, and I could carry the observation no further with this apparatus.

From the former scanty and imperfect expressions of the elasticity of the vapour of the condensed gases, Dove was led to put forth a suggestion*, whether it might not ultimately appear that the same addition of heat (expressed in degrees of the thermometer) caused the same additional increase of expansive force for all gases or vapours in contact with their liquids, provided the observation began with the same pressure in all. Thus to obtain the difference between forty-four and fifty atmospheres of pressure, either with steam or nitrous oxide, nearly the same number of degrees of heat were required; to obtain the difference between twenty and twenty-five atmospheres, either with steam or muriatic acid, the same number were required. Such a law would of course make the rate of increasing expansive force the same for all bodies, and the curve laid down for steam would apply to every other vapour. This, however, does not appear to be the case. That the force of the vapour increases in a geometrical ratio for equal increments of heat is true for all bodies, but the ratio is not the same for all. As far as observations upon the following substances, namely, water, sulphurous acid, cyanogen, ammonia, arseniuretted hydrogen, sulphuretted hydrogen, muriatic acid, carbonic acid, olefiant gas, &c., justify any conclusion respecting a general law, it would appear that the more volatile a body is, the more rapidly does the force of its vapour increase by further addition of heat, commencing at a given point of pressure for all; thus for an increase of pressure, from two to six atmospheres, the following number of degrees require to be added for the different bodies named: water 69°, sulphurous acid 63°, cyanogen 64°·5, ammonia 60°, arseniuretted hydrogen 54°, sulphuretted hydrogen 56°·5, muriatic acid 43°, carbonic acid 32°·5, nitrous oxide 30°; and though some of these numbers are not in the exact order, and in other cases, as of olefiant gas and nitrous oxide, the curves sometimes even cross each other, these circumstances are easily accounted for by the facts already stated of irregular composition and the inevitable errors of first results. There seems every reason therefore to expect that the increasing elasticity is directly as the volatility of the substance, and that by further and more correct observation of the forces, a general law may be deduced, by the aid of which, and only a single observation of the force of any vapour in contact with its fluid, its elasticity at any other temperature may be obtained.

Whether the same law may be expected to continue when the bodies approach near to the Cagniard de la Tour state is doubtful. That state comes on sooner in reference to the pressure required, according as the liquid is lighter and more expansible by heat and its vapour heavier, hence indeed the great reason for its facile assumption by ether. But though with ether, alcohol and water, that substance which is most volatile takes up this state with the lowest pressure, it does not follow that it should always be so; and in fact we know that ether takes up this state at a pressure between thirty-seven and thirty-eight atmospheres, whereas muriatic acid, nitrous oxide, carbonic acid and olefiant gas, which are far more volatile, sustain a higher pressure

* Poggendorff's Annalen, xxiii. 290; or Thomson on Heat and Electricity, p. 9.

than this without assuming that peculiar state, and whilst their vapours and liquids are still considerably different from each other. Now whether the curve which expresses the elastic force of the vapour of a given fluid for increasing temperatures continues undisturbed after that fluid has passed the CAGNIARD DE LA TOUR point or not is not known, and therefore it cannot well be anticipated whether the coming on of that state sooner or later with particular bodies will influence them in relation to the more general law referred to above.

The law already suggested gives great encouragement to the continuance of those efforts which are directed to the condensation of oxygen, hydrogen and nitrogen, by the attainment and application of lower temperatures than those yet applied. If to reduce carbonic acid from the pressure of two atmospheres to that of one, we require to abstract only about half the number of degrees that is necessary to produce the same effect with sulphurous acid, it is to be expected that a far less abstraction will suffice to produce the same effect with nitrogen or hydrogen, so that further diminution of temperature and improved apparatus for pressure, may very well be expected to give us these bodies in the liquid or solid state.

Royal Institution,
Feb. 19, 1845.

MAP
TO ACCOMPANY Mr AIRYS PAPER
ON THE TIDES
upon the
COAST OF IRELAND
Showing the Tide Stations and the Times of Tide Wave in Ireland
with the general lines of the Corresponding Tides in England and Scotland
Malin Hd
Inishtrahull
Fanad Hd
Tory Island
Rathlin Island
Mull of Cantire
Arran
Lough Foyle
Londonderry
Portrush
Giants Causeway
Larne
Island Magee
Antrim
Belfast Lough
Belfast
Carrickfergus
Lisburn
Mull of Galloway
Donaghadee
Isle of Man
Armagh
Newry
Dundalk
Drogheda
Howth
Kingstown
Dublin Bay
Wicklow Hd
Arklow
Anglesea
Bardsey Island
Bardsey
Tuskar
Wexford
Waterford
Dungarvan
Youghal
Cork
Queenstown Harbour
Kinsale
Old Head of Kinsale
Galley Head
Clear Island
Crookhaven
Mizen Hd
Valentia
Dingle Bay
Tralee
Limerick
Foynes
Kilrush
Loop Head
Galway
Aranmore
Inishbofin
Clifden
Slyne Head
Killary Bay
Westport
Clew Bay
Achill Island
Blacksod Bay
Broadhaven
Belmullet
Sligo
Donegal Bay
Killybegs
Rathlin O'Birne
Tory Island
Inishtrahull
Malin Hd
Scale of Miles
0 1 2 3 4 5 6 7 8 9 10 Miles

PHILOSOPHICAL

TRANSACTIONS

OF THE

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OF

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MDCCCXLV.

Adjudication of the Medals of the Royal Society for the year 1845 by the President and Council.

The Copley Medal to Professor Thomas Schwann of Louvain, for his Physiological Researches on the development of Animal and Vegetable Textures.

The Royal Medal, in the department of Astronomy, to George Biddell Airy, Esq., Astronomer Royal, for his Paper entitled "On the Laws of the Tides on the Coasts of Ireland, as inferred from an extensive series of observations made in connexion with the Ordnance Survey of Ireland," published in the Philosophical Transactions for 1845.

The Royal Medal, in the department of Physiology, to Thomas Snow Beck, Esq., for his Paper entitled "On the Nerves of the Uterus," communicated to the Society in 1845, and ordered for publication in the Philosophical Transactions.

The Paper appointed for the Bakerian Lecture in 1845, is Professor Daubeny's Memoir, entitled "On the Rotation of Crops."

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PHILOSOPHICAL TRANSACTIONS.

VII. *Memoir on the Rotation of Crops, and on the Quantity of Inorganic Matters abstracted from the Soil by various Plants under different circumstances.*

By CHARLES DAUBENY, *M.D., F.R.S., &c., Honorary Member of the Royal English Agricultural Society, and Professor of Rural Economy in the University of Oxford.*

Received May 5,—Read May 22, 1845.

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Part II.—On the chemical composition of certain crops cultivated in the Botanic Garden, and on the amount of inorganic principles abstracted by them from the soil during the period the experiments were conducted.

Part III.—On the chemical composition of the soil in which the crops were grown, and on the proportion of its ingredients that was available for the purposes of vegetation.

Introduction.

IN laying before this Society an account of certain experiments which I have undertaken with the view of elucidating the principles upon which the advantage of a rotation of crops in husbandry depends, it may be proper that I should in the first instance state the circumstances under which they were commenced, as well as those which led me during the course of them to deviate in some respects from my original plan of proceeding.

During the prosecution of a set of researches which embraced a period of more than ten years, it might naturally be expected, that the views at first entertained would become modified, and that arrangements deemed sufficient for carrying out the original plan should appear unsatisfactory, in proportion as glimpses of other truths, than those which enlightened us at the outset, began to open upon the field of our inquiry.

Thus, when first I determined to apply a portion of the ground at my disposal to experiments having reference to the rotation of crops, the scientific world was ge-

nerally impressed in favour of a theory which the celebrated De Candolle had so ingeniously and eloquently maintained; namely, that a soil became unfitted for supporting a second crop of any given plant, in consequence of the deleterious influence exerted upon it by juices excreted from the former one.

My original object therefore was,—first, to detect, if possible, the chemical nature of these supposed excretions; and secondly, to demonstrate their poisonous influence, by taking account of the expected diminution in the amount of the crop exposed to them, beyond that of another in which all the circumstances were the same, excepting the presence of the excretions in question.

To accomplish these two objects, it seemed sufficient to set apart a number of plots of ground uniform as to the quality and richness of its soil, planting one-half of the number year after year with the same species of crop until the land no longer produced it, and the other moiety with crops of the same description, succeeding one another in such a manner, that no one plot should receive the same twice during the period of the continuance of these experiments, or at least within a short interval of one another.

By weighing the produce of each plot, reduced to the same uniform condition of dryness, when it had arrived at maturity, I hoped to obtain data for computing, how much of the expected diminution might be referred to the exhaustion of the ground, and how much to the effect of excretions which the preceding crop had given out.

The influence of seasons indeed is in all these cases one of the most important elements in the calculation, yet by taking the average of a number of years, it was hoped that this source of error might be eliminated, and that whilst the mean of the crop obtained during the latter half of the period, as compared with that of the former half, might suggest the rate of exhaustion brought about by the annual demand made upon the resources of the soil, the difference between the *permanent* and the *shifting* crop in each instance might tend to show, in what degree the excretory function of each plant contributed to the result.

Assuming, therefore, on the faith of the then existing authorities, that soil would soon become deficient in the food which was required for the plants grown in it, and moreover that, even if not exhausted, it would become unsuitable to their growth, by being contaminated with the excretions from preceding crops, I conceived it unnecessary either to undertake an analysis of the soil itself at the commencement of my labours, or to inquire into the chemical constitution of the crops which I had obtained in the course of them.

Supposing, as was then too hastily assumed, that the composition of each vegetable was uniform, and had been already determined with sufficient precision, it should follow, that the amount of produce ought in itself to be an index of the quantity of inorganic matter abstracted from the soil, and that the number of crops obtained before the soil became effete would indicate the relative richness of the latter in those ingredients which were essential to the growth of the plant in question.

As I proceeded however in my experiments, I began to find, that both the postulates on which I had built were unsound, for neither was I able to detect any foreign organic matter in the soil, referable to the excretions of the crop which had grown in it*, nor did I find that uniform difference between the shifting and the permanent crop, to the disadvantage of the latter, which I should have expected upon the principles of De Candolle's theory.

Moreover, the researches of Braconnot, which have since been made known to the world, tended still further to throw doubt upon the truth of the facts on which the doctrine of excretion reposed, and when no longer swayed by the authority of the distinguished author of the theory in question, I perceived more clearly the difficulty of reconciling it with many facts or opinions that seemed current amongst agriculturists—such, for instance, as the growth of repeated crops of the most exhausting plants in certain rich alluvial, or newly settled countries; the continuance of a plant in a state of nature for ages in the same locality; and lastly, the views of Liebig, which went to prove, that the food of plants, so far as their organic constituents are concerned, is derived in all instances from the elements of air and of water.

No sooner, therefore, had I become suspicious as to the truth of the opinion which I had previously entertained as to the excretions from the roots of plants being capable of explaining the falling off of a crop after repetition, than I felt desirous of shaping my inquiries in such a manner as to ascertain, if possible, which of the other two conceivable explanations might deserve a preference; whether, for instance, the falling off of the crop was attributable to a failure in the soil of organic matters fitted for its nutrition, or of those inorganic materials which it equally required.

* The soils that seemed to me most likely to afford indications of the presence of root excretions were those which had reared crops of poppies and of tobacco for several years in succession, the former plant containing, in morphia and meconic acid, products readily recognizable by chemical tests, and the latter one sufficiently so in nicotine.

I accordingly digested sifted portions of the soils, amounting in each instance to 5 lbs., in water for several hours.

The water drained off was evaporated, and then filtered.

The clear solution was first treated with sugar of lead, and the precipitate which fell was collected, and then dissolved in water acidulated with sulphuric acid. Had any meconic acid existed in combination with the lead, it would have been thus separated, the metal being precipitated along with the sulphuric acid with which it forms an insoluble salt.

None of the tests, however, usually employed for detecting meconic acid produced any effect,—chloride of iron dissolved in alcohol causing no red colour, and ammonio-sulphate of copper not being rendered green.

The liquor remaining after the introduction of the sugar of lead might have contained morphia held in solution by acetic acid. To detect it, the lead was in the first place thrown down by sulphuretted hydrogen, after removing which, the remaining solution, after being concentrated, was treated with ammonia, which produced a flocculent precipitate.

This, however, proved destitute of morphia, for neither was there any blue colour as produced by chloride of iron, nor any redness by nitric acid.

My attempts to detect nicotine in the soil in which tobacco had been grown proved equally ineffectual.

To determine this, it seemed necessary to appreciate, if possible, first, what materials the soil might have contained, both before the experiments commenced, and after their termination; and secondly, what might be the constitution of the plants themselves both in the permanent and the shifting crop, as compared with the normal condition of the same.

But as the experiments which I had instituted extended to no less than sixteen different species, my object being to select at least one out of each natural family, which contained amongst the plants included under it any of those usually cultivated for farm or garden purposes in this country, it seemed necessary to limit that part of the inquiry which involved the necessity of ash analysis to a portion only of the series, and accordingly, in the autumn of 1844, I selected from the crops grown in that year the following as the subjects of chemical examination, namely, Barley, Potatoes, Turnips, Flax, Hemp, and Beans.

Of each of these six plants, the shifting and permanent crops, after having been weighed in the usual manner, in order to estimate their relative amount, were reduced to ashes, so that the proportion of inorganic to organic matter might in the first instance be determined.

In consequence of the largeness of the bulk, iron vessels were necessarily employed for burning away the volatilizable parts, and hence a portion of peroxide of iron was always introduced into the ashes, which, being indefinite in quantity, rendered it necessary for me, in the subsequent analyses, to regard the whole of that ingredient as extraneous, and to reject it from the calculation. The same course was also pursued with respect to a certain variable amount of sand and charcoal always present in the ash, the former derived evidently from the soil, the latter from the carbonaceous matter of the vegetable, which could not be entirely removed by the combustion.

Of each of these six plants it appeared necessary to analyse at least three specimens—the first taken from the permanent crop, the second from the shifting one, the third from a piece of ground, not belonging to the spot at which the experiments were carried on, and under ordinary treatment, but corresponding as nearly as possible in natural character to the soil of the experimental garden.

Thus this part alone of the inquiry involved at least eighteen distinct analyses, an amount of labour, which, as I soon found, my other occupations precluded me from undertaking, and which I was glad to delegate to other hands.

I therefore esteemed myself fortunate in being able to secure the services of Mr. Thomas Way, a gentleman, who had for the last two years officiated as Assistant to Professor Graham of University College, London, and who was recommended to me by that distinguished chemist as well-qualified for the task.

On him, therefore, the merit, as well as the responsibility, of this part of the inquiry must mainly devolve; all that I can lay claim to in this part of the subject as my own, is the having considered, in conjunction with him, the method of analysis which

he ultimately adopted, and having made such preliminary trials on one of the crops which he afterwards analysed, as tended to satisfy me, that on those points in which the plan differs from that proposed by Dr. WILL, our method deserves the preference, on the score of convenience, if not with respect to accuracy.

In a case of this kind, experience alone can determine the degree of confidence which is due to the results obtained, but I ought not to withhold my own individual testimony to their fidelity, from having witnessed the manner in which they were conducted by Mr. WAY, his perfect familiarity with the processes which he pursued, and the scrupulous care taken by him in repeating every step in the investigation, which presented anomalous results, or appeared from any cause open to suspicion.

But to complete my design, an analysis of the soil, as well as of the crops which grew in it, was requisite, and to this subject therefore my attention was next directed.

Now, when we consider the nature of a soil in an agricultural point of view, or in reference to its suitableness for the growth of various kinds of vegetables, two questions naturally come before us; namely, what amount of ingredients capable of being assimilated in the course of time by the crops does it contain; and secondly, what is the amount of those which are present, in a condition actually available for their purposes, at the precise moment when the examination is undertaken.

Both the above points are obviously quite distinct from that of the total amount of ingredients actually existing in the soil, and hence some might be disposed to add to the labour of the two preceding investigations, that of ascertaining the whole of its constituents, whether in a state to be affected by the ordinary agents of decomposition, or not.

The latter question, however, seems to me to possess, with reference to the agriculturist, only a speculative interest, and when introduced into a Report intended for his use, may be more liable to mislead than to instruct, unless due caution be taken to point out to him, how much of each ingredient is to be regarded as inert, and how much of it as applicable to the future or present uses of the plant.

Let us take the case of a natural soil, composed of certain kinds of disintegrated lava, or even of granite, in which it is evident, that an actual analysis, conducted by means of fusion with barytes, or lead, or by those other processes which chemists employ for decomposing compounds of a refractory nature, would detect the presence of a large per-centage of alkali, not improbably of a certain amount of phosphate of lime, and in short would indicate an exuberant supply of all those ingredients which plants require for their support. Nevertheless a soil of this description, in consequence of the close union of the elementary matters of which it consists, and of the compactness of its mechanical texture, might be as barren, and as incapable of imparting food to plants, as an artificial soil composed of pounded glass is known to be, notwithstanding the large proportion of alkali which it contains.

Thus I have myself observed*, that the soil which covers the serpentine rock of Cornwall, a mineral consisting of—

Silica	43·07
Magnesia	40·37
Alumina	0·25
Lime	0·50
Oxide of iron	1·17
Water	12·45—Hisinger,

contains so minute a proportion of magnesia, that in an analysis of a small sample I altogether overlooked its presence, in so great a degree does the mechanical texture of the rock, and the state of combination subsisting between its ingredients, preserve it from the decomposing action of the elements which tend to set loose its treasures.

Now it seems obvious, that whatever cannot be extracted from a soil by digestion in muriatic acid during four or five successive hours, must be in such a state of combination as will render it wholly incapable of imparting anything to a plant, for such a period of time at least as can enter into the calculations of the agriculturist; and moreover, that all which muriatic acid extracts, but which water impregnated with carbonic acid fails in dissolving, ought to be regarded as at present contributing nothing, although it may ultimately become available to its purposes.

I have therefore thought proper to distinguish between the actually available resources of the soil, and those ultimately applicable to the uses of the plant, designating the former as its *dormant*, and the latter as its *active* ingredients.

The portion dissolved after digestion in muriatic acid will contain both the dormant and the active; that taken up by water impregnated with carbonic acid will consist merely of the latter; the difference in amount between the two will therefore indicate the dormant portion of its contents.

The dormant and active portions may both be comprehended under the designation of its available constituents, whilst those which, from their state of combination in the mass, can never be expected to contribute to the growth of plants, may be denominated the passive ones.

Every soil, which is capable of yielding an abundant crop of any kind of plant after fallowing, must be assumed to possess in itself an adequate supply of all the ingredients necessary for its support in an available condition, but it is plain that these could not have existed in an active one, or such an interval of rest would not have been required for rendering them efficient.

Accordingly it is quite possible, that after ten years cropping, the soil of the experimental garden might still retain plenty of alkaline salts and phosphates, although what was ready to be applied to the uses of the plant had for the most part been absorbed by the crops previously obtained.

* Lecture on the Application of Science to Agriculture, from the Journal of the Royal Agricultural Society of England, vol. iii. part 1.

With a view then to this part of the inquiry, I proposed to estimate, first, the amount of ingredients severally present in the soil which might sooner or later become available for the purposes of vegetation; and secondly, that of those principles which were in a state to be applied immediately to those uses. It would also have been instructive, to determine, by a comparative analysis of the soil, in the state in which it was before, and after the experiments had been instituted, the loss which had been occasioned by the crops in both these particulars; but as, from the reasons assigned, I had neglected to examine the identical soil of the experimental garden before the researches commenced, I was obliged to content myself with obtaining an approximation to its probable constitution, by selecting for examination that taken from a portion of the garden, which was immediately contiguous, but which had recently been manured, and had borne good crops.

Here also I was assisted by Mr. WAY, who undertook the more laborious part of the inquiry, namely, that of determining the entire amount of the available constituents present in certain of the soils, leaving to me the task of ascertaining merely the nature of those which could be extracted by water.

The investigation therefore divides itself naturally into three heads; the first respecting the actual amount of the permanent and shifting crops each year obtained; the second, the chemical constitution of the ashes resulting from those which had been burnt for the purpose of examination; and the third, the nature of the actual as well as of the available ingredients of the soil in which the crops had been reared.

PART I.

On the quantity of produce obtained from the several plots of ground, each year throughout the period during which the experiments were continued.

The following plants were made the subjects of experiment:—

Spurge	*Euphorbia lathyris*	from 1835 to 1838
Potatoes	*Solanum tuberosum*	from 1834 to 1844
Barley	*Hordeum sativum*	from 1835 to 1844
Turnips	*Brassica rapa*	from 1834 to 1844
Hemp	*Cannabis sativa*	from 1835 to 1844
Flax	*Linum usitatissimum*	from 1835 to 1844
Beans	*Vicia faba*	from 1835 to 1844
Tobacco	*Nicotiana Tabacum*	from 1834 to 1844
Poppies	*Papaver somniferum*	from 1834 to 1844
Buckwheat	*Polygonum fagopyrum*	from 1835 to 1844
Clover	*Trifolium pratense*	from 1834 to 1844
Oats	*Avena sativa*	from 1839 to 1844
Beet	*Beta vulgaris*	from 1839 to 1844
Mint	*Mentha viridis*	from 1835 to 1844
Endive	*Cichorium endivia*	from 1835 to 1844
Parsley	*Apium petroselinum*	from 1835 to 1844

As the experiments were carried on from 1834 to 1844 inclusive, it may be satisfactory to state in the first instance such of the meteorological characters of these years as may be gathered from the Register kept at the Radcliffe Observatory, relative to the mean temperature, or from my own observations made at the Botanic Garden, Oxford, as to the amount of rain.

Year.	Inches of Rain.	Where observed.	Variation + or − (above or below) the mean of the period.	Mean temperature of the year.	Variation + or − (above or below) the mean of the period.
1834.	21·899	Obs.	− 2·845	52·8	+2·282
1835.	26·182	Obs.	+ 1·438	51·5	+0·982
1836.	24·339	Obs.	− 0·365	49·9	−0·618
1837.	21·900	B.G.	− 2·844	50·0	−0·518
1838.	20·080	B.G.	− 4·664	49·9	−0·618
1839.	32·720	B.G.	+ 7·976	51·2	−0·318
1840.	18·530	B.G.	− 6·214	49·9	−0·618
1841.	35·275	B.G.	+10·531	49·8	−0·718
1842.	23·490	B.G.	− 1·254	50·8	+0·282
1843.	25·150	B.G.	+ 0·406	50·3	−0·218
1844.	22·621	B.G.	− 2·123	49·6	−0·918
Total in eleven years.	272·186	....	....	555·7	
Average of eleven years.	24·744	....	....	50·518	

The plots of ground set apart for the experiments were not exactly equal in point of size, but their square contents being known, it was easy to reduce the crops to one standard, and that of 100 square feet was selected as the most convenient.

In reporting to the Society the results, I shall therefore always suppose that reduction as being made, and shall set down what ought to have been the produce, supposing each plot to have measured exactly 100 square feet. In this statement I will begin with the only case in the whole series to which De Cándolle's theory of excretions appears at all applicable; namely, that in which the plant experimented on was a species of Spurge, the *Euphorbia lathyris*.

In 1835 a luxuriant crop of this weed was obtained, amounting to about 18 lbs., but the next year the produce had dwindled almost to nothing, and in 1837, in which fresh plants were introduced, an equal failure took place.

Nor did any new plants start up in 1838, so that in 1839 the plot was sown with flax, barley, and beans, of all of which I obtained a tolerable yield.

This experiment therefore might be appealed to in support of De Candolle's views, as it would appear, that excretions had been emitted from the roots of the Euphorbia, which proved injurious to plants of the same species as those from which they had proceeded, but which exerted no such poisonous influence upon others not allied to them in organization; or, if it be objected, that during the course of 1838 the excretions might have become so far decomposed as to lose their poisonous cha-

racter, still the failure of the second and third crops of Euphorbia would seem attributable to some deleterious influence exerted by the excretions of the antecedent crop, rather than to the ground having become exhausted, inasmuch as the latter, without being in the meanwhile enriched with manure, proved its ability to produce tolerable crops of other vegetables.

The acrid nature of the juices of the Euphorbia may possibly explain, why this plant should constitute an apparent exception to the rest, for it will be seen, that in all the other cases, the diminution in the amount of produce, consequent upon the continuation of the crop from year to year, was only such as might be supposed to result from a falling off in some one of those ingredients which were necessary for its development, and was not of a nature to indicate the existence of anything poisonous in the soil in which it grew*.

I shall therefore now proceed to state the amount of produce obtained during the several years from each of the remaining plants above enumerated, distinguishing the crop which was repeated year after year in the same plot of ground as the *permanent* one, and that which was grown successively in different parts of the garden as the *shifting* one.

1. *Solanum tuberosum.*—Ground not manured since 1833.

Year.	Permanent crop. Weighed without having been dried, but merely cleaned from dirt.	Shifting crop.
	Plot No. 1.—After a crop of Turnips.	After a crop of *Papaver somniferum.*
1836.	Tubers . . . 89·50	Tubers . . 84·00
1837.	Tubers . . . 59·5	After a crop of *Cannabis sativa.* Tubers . . 108·0
1838.	Tubers . . . 68·0	After a crop of *Cannabis sativa.* Tubers . . 68·5
1839.	Tubers . . . 59·0	After a crop of *Polygonum fagopyrum.* Tubers . . 132·0

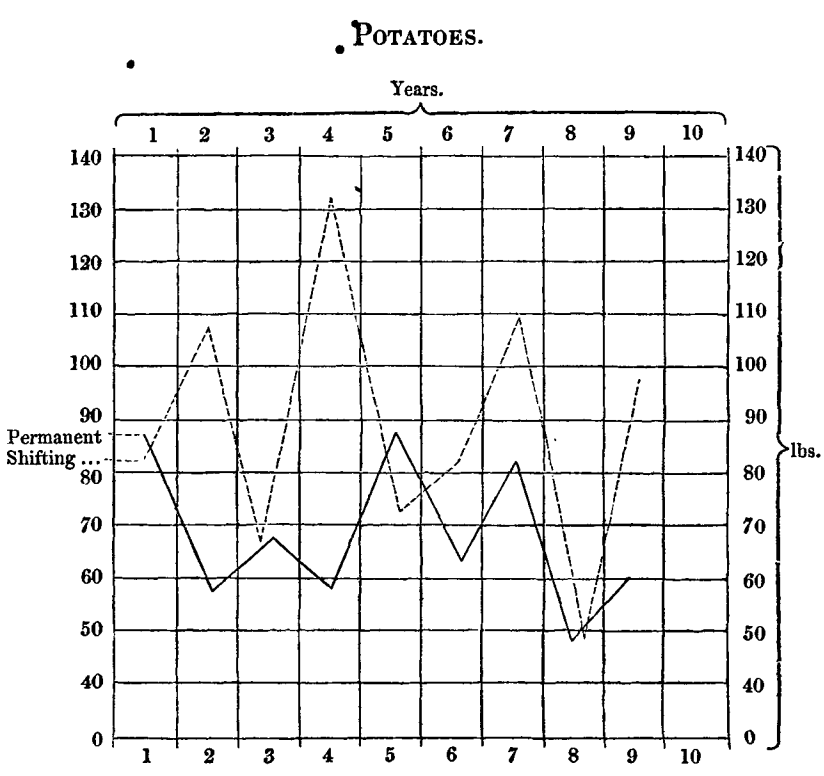
1840. In this year it occurred to me, that it might be interesting to determine what difference, as to the amount of produce, would be produced by planting in one instance tubers from the crop obtained the year before in the same plot, and by employing in another those raised in some different locality.

The difference in quantity will be seen not to be very considerable.

* It can hardly, I think, be denied, that juices are excreted from the roots, as well as taken up by them; the only question is, are these excretions injurious to the plants of the same species which grew in the soil afterwards, and if they are, are they favourable to the growth of others? The first of these positions may be countenanced by the facts detailed in the text, but the latter is little, if at all, corroborated by them.

Year.	Permanent crop. No. 1.—Tubers from the same plot planted.	Permanent crop. No. 3.—Tubers from a different locality planted.	Shifting crop.
		After *Euphorbia lathyris*, which had failed.	After *Linum usitatissimum.*
1840.	Tubers . . 88½	Tubers . . 90	Tubers . . 72
			After *Linum usitatissimum.*
1841.	Tubers . . 64	Tubers . . 87½	Tubers . . 81½
			After *Vicia Faba.*
1842.	Tubers . . 82·5	Tubers . . 94·0	Tubers . . 110
			After *Vicia sativa.*
1843.	Tubers . . 48·6	Tubers . . 38·6	Tubers . . 48·4
			After *Hordeum sativum.*
1844.	Tubers . . 61·0	Tubers . . 57¾	Tubers . . 98·0
	Average of nine years.		Average of nine years.
	Tubers . . 68·9	. . . . .	Tubers . . 89·1
	Average of the first five years.	Average of five years.	Average of the first five years.
	Tubers . . 72·9	Tubers . . 74·77	Tubers . . 92·8

The following diagram may convey a clearer idea of the differences in the amount of the crops, and the relation between the permanent and shifting one.



It is worth remarking, that the average of the shifting crops of potatoes corresponded very nearly to the amount obtained in the year 1844, from a bed of similar size, in a portion of the garden contiguous to that on which my experiments had been carried on, and which had been recently manured, the produce in this instance being 96·0 lbs., whilst the average of nine years in the other case was 89·1 lbs., and moreover that the produce of the last year, in which the crop succeeded one of barley, was not less than 98 lbs., thus apparently showing, that after so long a period of cropping, there was still a sufficient supply in the soil of those ingredients which were requisite for the due development of the plant.

An examination of samples of potatoes from the crop of 1844 proved, that the shifting crop contained more starch, and more of the woody fibre and other organic matters which belong to this vegetable, than either of the permanent ones; and that of the latter, the one grown in soil which had borne potatoes only five years, approached in these respects more nearly to the shifting, than that taken from the soil which had been cropped during ten years. The proportion of water in the two cases was not very different, but with respect to the inorganic matters, it was found that the remark held good.

Here the shifting crop yielded in a given amount of tubers the least, and the permanent crop the largest quantity of ash, as if the deficiency of organic matter had been made up by an increased quantity of that which was derived from inorganic sources. The nature of this latter portion will be stated when I proceed to detail the analyses performed by Mr. Way.

On examining a specimen of a good mealy description of potatoe taken from a garden in the neighbourhood of Oxford, the soil of which was similar to that of the experimental garden, I found the proportion of starch much the same as in the shifting crop, but the quantity of water greater.

The following is a tabular view of the results:—

	Starch.	Fibrous matter.	Other solid matters.	Water.
	per cent.			
Good mealy kind of potatoe from a garden near Oxford . .	13·00	5·90	5·6	75·5
Shifting crop in experimental garden	13·67	9·76	5·7	71·9
Permanent crop of five years' standing.	10·54	11·32	4·5	73·7
Permanent crop of nine years' standing	9·11	9·76	8·8	72·4

2. *Hordeum sativum.*

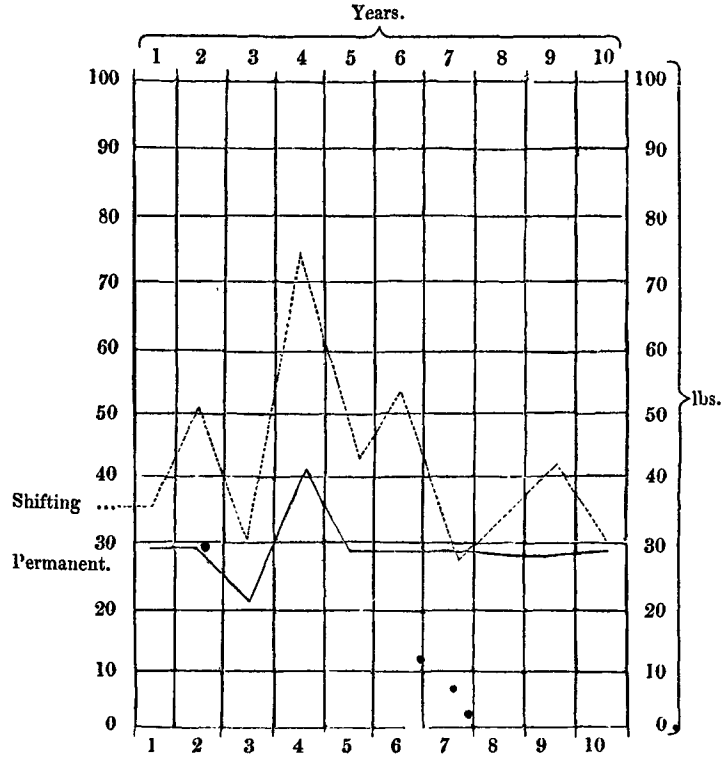
The next crop I shall notice is Barley, in which it will be perceived that, allowing for differences of seasons, the produce obtained during ten successive years was tolerably uniform, there being however a considerable balance in favour of the shifting over the permanent crop.

The following are the results:—

Year.	Permanent crop. Produce in a dried state.	Shifting crop. Produce in a dried state.
	No. 4.	After *Linum usitatissimum.*
1835.	29·0	35·5
		After *Apium petroselinum.*
1836.	29·0	51·0
		After *Linum usitatissimum.*
1837.	21·5	30·0
		After *Cichorium endivia.*
1838.	42·5	75·0
		After *Brassica Rapa.*
1839.	28·0	41·0
		After *Brassica Rapa.*
1840.	28·75	54·5
		After *Vicia Faba.*
1841.	28·0	27·4
		After *Solanum tuberosum.*
1842.	27·5	34·2
		After *Papaver somniferum.*
1843.	26·5	42·5
		After *Cannabis sativa.*
1844.	28·7	30·0
	Average of ten years . . . 28·9	Average of ten years . . . 42·1
	Average of first five years . 30·0	Average of first five years . 46·5
	Average of last five years . 27·8	Average of last five years . 37·7
	Maximum in one year . . 42·5	Maximum in one year . . 75·0
	Minimum in one year . . 21·5	Minimum in one year . . 30·0

The following Table will show the curve of their growth :—

BARLEY.



The quantity of barley produced in a contiguous piece of ground recently manured, was 39·4 lbs., being more than the average of the shifting crops, but about equal to the produce obtained the first year from the permanent one.

3. *Brassica Rapa.*

Next in the series are Turnips, in which the difference between the average of the shifting and permanent crop is very remarkable, namely as 176 to 100.

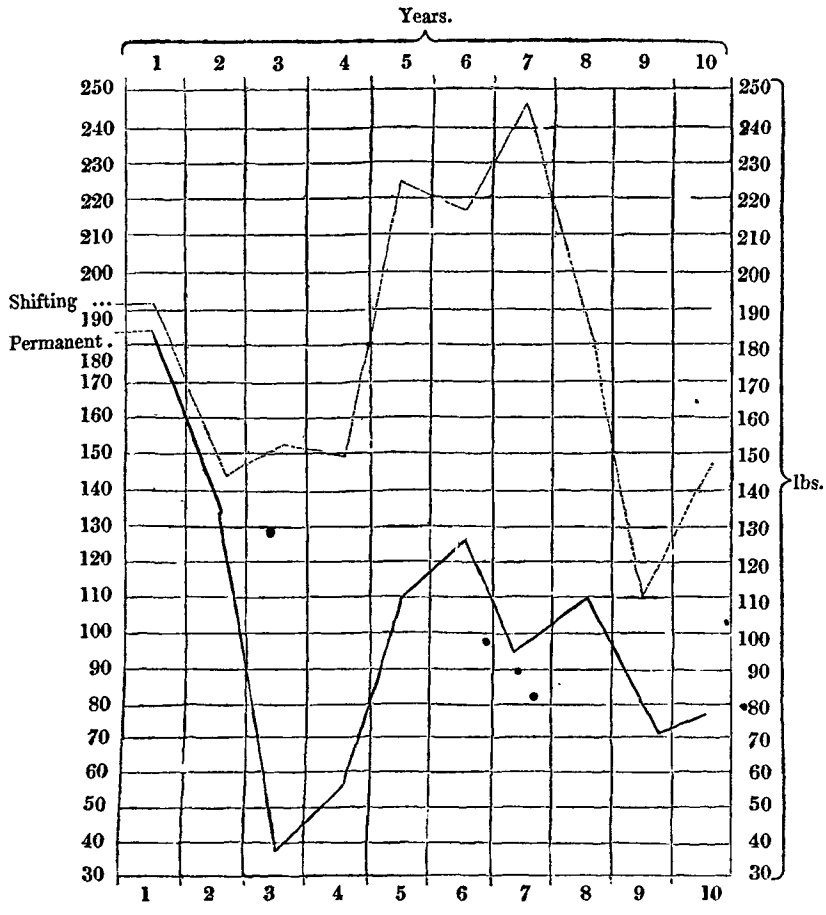
Nevertheless it will be seen, that even the latter did not sink below a certain amount when the seasons were tolerably favourable.

The following are the results:—

Year.	Permanent crop. Produce in a green state.	Shifting crop.
	No. 2.	
1834.	125·0	
1835.	185·0	192·0
		After *Linum usitatissimum.*
1836.	133·0	144·0
		After *Vicia Faba.*
1837.	37·2	154·0
		After *Linum usitatissimum.*
1838.	56·0	150·0
		After *Cichorium endivia.*
1839.	110·0	226·0
		After *Nicotiana rustica.*
1840.	128·0	217·0
		After *Papaver somniferum.*
1841.	98·0	245·0
		After *Avena sativa.*
1842.	111·0	179·0
		After *Solanum tuberosum.*
1843.	73·5	110·0
		After *Helianthus annuus.*
1844.	77·0	148·0
	Average of ten years . . 100·8	Average of ten years . . 176·5
	Average of first five years 104·0	Average of first five years . 173·0
	Average of last five years . 97·5	Average of last five years . 176·5
	Maximum in one year . . 185·0	Maximum in one year . . 245·0
	Minimum in one year . . 37·2	Minimum in one year . . 110·0

The following diagram will show the variations in their yearly produce :—

TURNIPS.



In the contiguous bed recently manured the produce was 152 lbs., a quantity about intermediate between the average of the shifting and permanent crops.

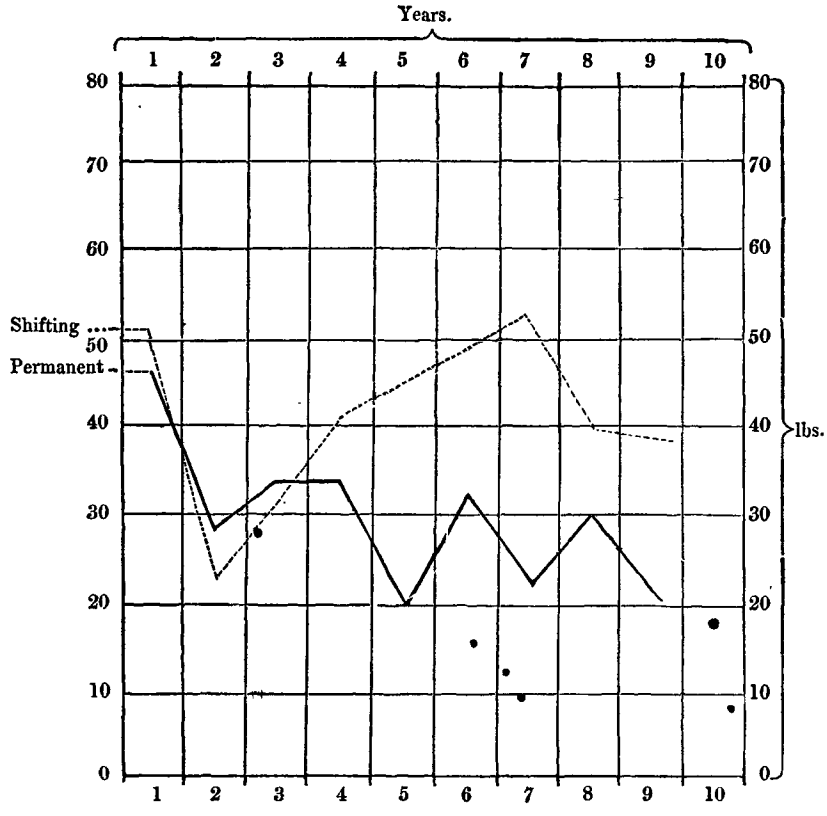
4. *Cannabis sativa.*

The next crop I shall notice, Hemp, presents a very uniform rate of produce during the whole period, nevertheless the shifting crop, which however was not grown during the years 1840 and 1841, presents a much larger produce than the permanent one, as will be seen by the following Table.

Year.	Permanent crop. Dried in the sun.	Shifting crop. Dried in the sun.
	No. 17.	After *Trifolium pratense.*
1836.	46·5	52·5
		After *Brassica Rapa.*
1837.	27·75	22·5
		After *Brassica Rapa.*
1838.	34·0	32·5
		After *Hordeum sativum.*
1839.	34·0	42·5
1840.	20·5	None.
1841.	33·3	None.
		After *Beta vulgaris.*
1842.	23·7	53·0
		After *Polygonum fagopyrum.*
1843.	30·0	39·5
		After *Brassica Rapa.*
1844.	21·5	37·6
	Average of nine years . . 30·13	
	Average of first five years 32·55	
	Average of last four years 27·12	Average of four years . . 30·0
	Maximum in one year . 46·50	Maximum in one year . . 52·5
	Minimum in one year . . 20·50	Minimum in one year . . 22·5

The following diagram will show the variations in the yearly produce:—

HEMP.



In the contiguous bed recently manured the crop weighed 45·4 lbs., somewhat more than the average of the shifting, and considerably exceeding that of the permanent beds.

5. *Linum usitatissimum.*

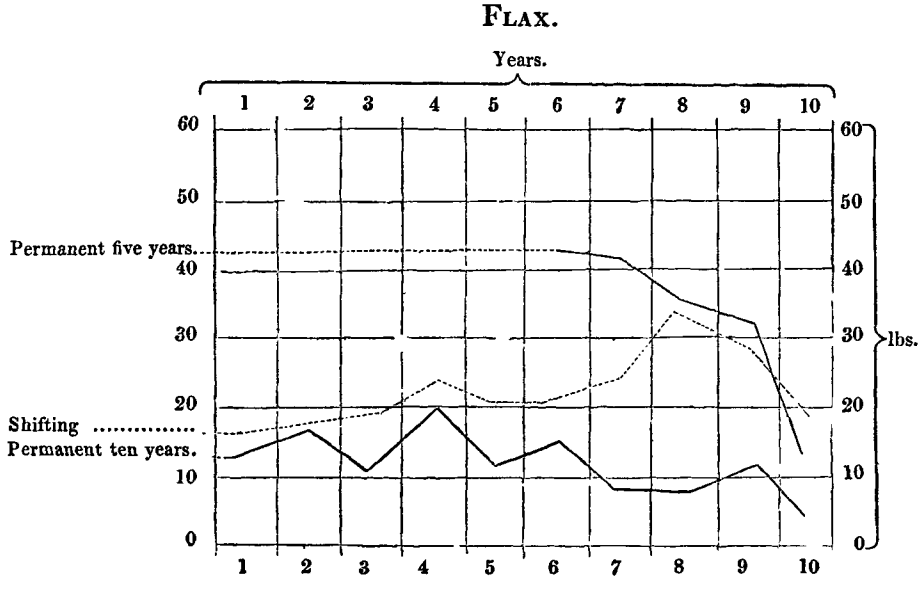
Flax presents a gradual, though not an uniform, rate of deterioration, the shifting crop always standing in advance of the permanent one.

In this instance I tried the same experiment as in the case of the potatoes, namely, that of sowing one bed with seed from the last year's crop, and the second with seed obtained from some other source. The latter produced much the most abundant crop, but I am now inclined to attribute its superiority chiefly to its succeeding a crop of Valerian, a plant which probably draws little from the soil, and which consequently having grown in it for five successive years, had given time to the materials of the earth to undergo decomposition, so that an accumulation of nutritious principles may have taken place in it, nearly as would have been the case if it had been left entirely fallow.

The following are the results obtained :—

Year.	Permanent crop of ten years' standing. Crop dried.	Permanent crop of five years, fresh seed. Crop dried.	Shifting crop. Crop dried.
	No. 19.		
1835.	12·9		15·8
			After *Polygonum fagopyrum.*
1836.	16·7	. . . . .	17·0
			After *Solanum tuberosum.*
1837.	12·0	. . . . .	19·8
			After *Solanum tuberosum.*
1838.	20·0	. . . . .	25·5
			After *Valeriana Phu.*
1839.	13·75	. . . . .	21·75
		Nos. 13 and 14.	
		After *Valeriana Phu.*	After *Polygonum fagopyrum.*
1840.	16·5	43·5	21·5
			After *Vicia sativa.*
1841.	9·0	41·0	25·0
			After *Brassica Rapa.*
1842.	8·4	34·0	34·0
			After *Vicia Faba.*
1843.	11·7	31·0	29·6
			After *Solanum tuberosum.*
1844.	5·2	11·5	17·8
	Average of ten years . . . 12·6	Average of five years . . . 32·5	Average of ten years . . . 22·7
	Average of first five years 15·0	. . . . .	Average of first five years 19·9
	Average of last five years 10·4	. . . . .	Average of last five years 25·5
	Maximum in one year . . 20·0	Maximum in one year . . 43·5	Maximum in one year . . 34·0
	Minimum in one year . . 5·0	Minimum in one year . . 11·5	Minimum in one year . . 15·8

The following diagram will show the curve of their growth :—



The contiguous manured crop weighed 22·2, rather less than the average of the shifting crops.

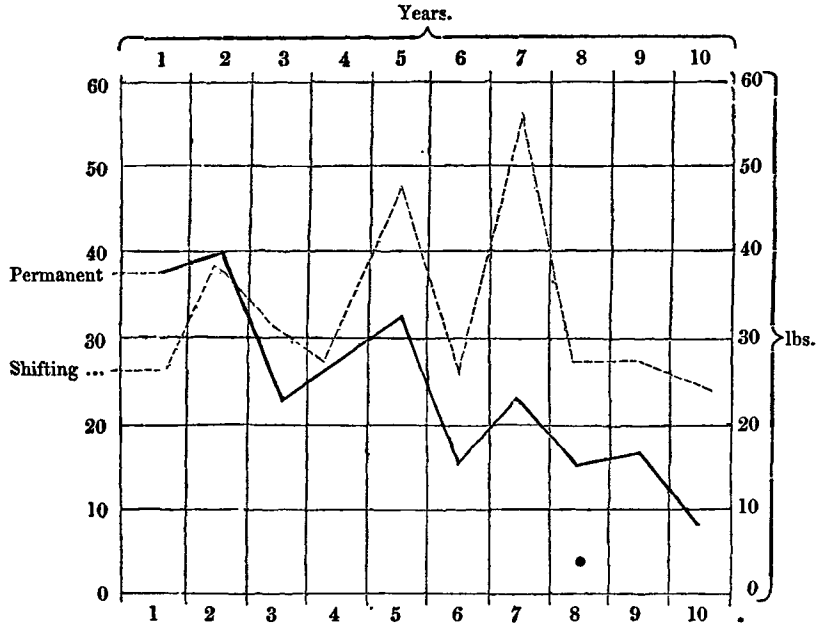
6. *Vicia Faba.*

Beans showed a considerable falling off in the case of the permanent crop, but not in the shifting, the following being the results obtained:—

Year.	Permanent crop. Produce in a dried state.	Shifting crop. Produce in a dried state.
	No. 23.	
1835.	38·0	27·7
		After *Linum usitatissimum.*
1836.	40·0	39·0
		After *Euphorbia lathyris.*
1837.	23·5	31·2
		After *Nicotiana rustica.*
1838.	28·4	28·5
		After *Nicotiana rustica.*
1839.	34·0	48·0
		After *Papaver somniferum.*
1840.	16·5	26·5
		After *Polygonum fagopyrum.*
1841.	24·5	56·0
		After *Papaver somniferum.*
1842.	15·8	28·0
		After *Avena sativa.*
1843.	17·1	28·0
		After *Nicotiana rustica.*
1844.	9·2	24·0
	Average of ten years . . . 24·7	Average of ten years . . 33·6
	Average of first five years . 32·8	Average of first five years . 34·8
	Average of last five years . 16·6	Average of last five years . 32·5
	Maximum in one year . . 40·0	Maximum in one year . . 56·0
	Minimum in one year . . 9·2	Minimum in one year . . 24·0

The following represents the curve of their growth :—

BEANS.



In the contiguous manured bed the crop was only 27·1 lbs., not greatly exceeding the average of the permanent, and falling considerably short of that of the shifting crops.

7. *Nicotiana rustica.*

Tobacco is one of the plants which most strikingly illustrates the dependence of the crop upon manuring; the first year the produce being 178 lbs., whilst it sunk in six years' time to 17 lbs. The whole of this diminution, however, must not be set down to the deficiency of inorganic matter, since in subsequent years the produce became greater, although it never recovered its former rate.

In this instance the permanent presents a higher average than the shifting crop, but this seems attributable to the circumstance that in the latter instance the soil had been previously drawn upon by a crop of beans, whilst in the former it had been recently manured.

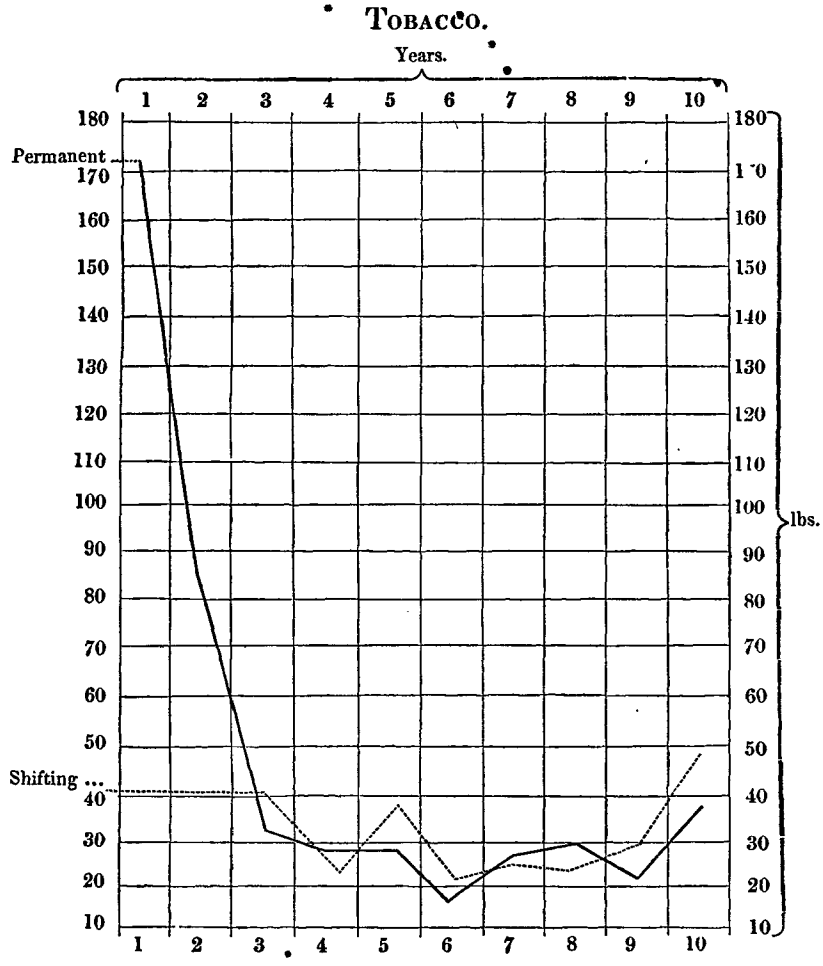
The following are the results :—

Year.	Permanent crop. Produce nearly dry.	Shifting crop. Produce nearly dry.
	No. 9*a*.	
1834.	172·0	None.
1835.	83·0	None.
1836.	33·0	After *Apium petroselinum.* 42·0
1837.	29·0	After *Papaver somniferum.* 25·0
1838.	29·0	After *Euphorbia lathyris.* 38·0

Nicotiana rustica (Continued).

Year.	Permanent crop. Produce nearly dry.	Shifting crop. Produce nearly dry.
	No. 9*a*.	After *Linum usitatissimum.*
1839.	17·25	22·5
		After *Cichorium endivia.*
1840.	27·0	25·0
		After *Apium petroselinum.*
1841.	30·0	24·0
		After *Helianthus annuus.*
1842.	22·5	30·0
		After *Brassica Rapa.*
1843.	37·8	49·0
	Average of ten years . . . 48·0	Average of eight years . . 32·0
	Average of first five years . 69·2	Average of first four years . 32·0
	Average of last five years . 26·9	Average of last four years . 32·0
	Maximum in one year . . 172·0	Maximum in one year . . 49·0
	Minimum in one year . . 17·2	Minimum in one year . . 22·5

The following diagram shows the variations of the crops :—



8. *Papaver somniferum.*

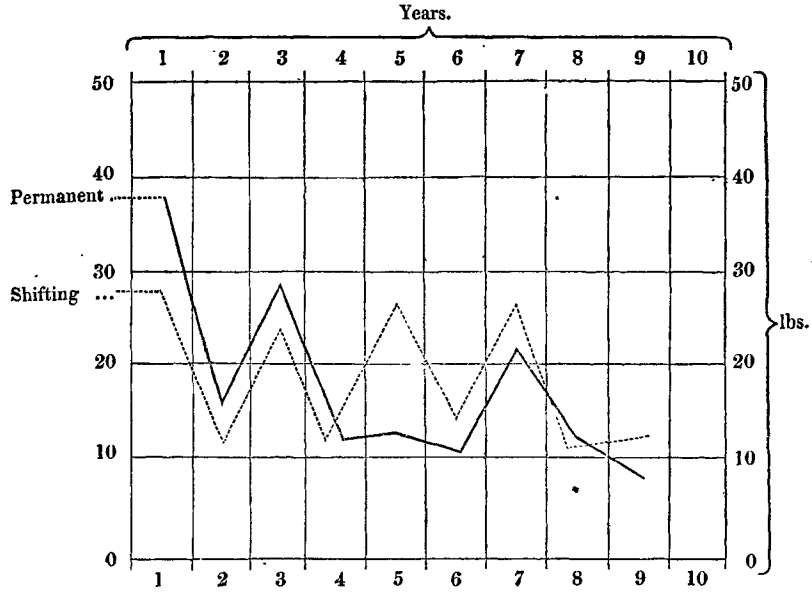
Poppies, although considered an exhausting crop, are said to be frequently grown for years in succession without any apparent decrease, but the results of my experiments serve to show, that this will not happen unless they are occasionally manured. It is remarkable how near in this instance the average of the shifting and permanent crops approaches each other.

The following Table shows the results:—

Year.	Permanent crop. Dried in the sun.	Shifting crop. Dried in the sun.
	No. 5.	After *Euphorbia lathyris.*
1836.	38·0	27·0
		After *Cichorium endivia.*
1837.	15·5	11·0
		After *Polygonum fagopyrum.*
1838.	29·0	24·0
		After *Solanum tuberosum.*
1839.	13·0	13·0
		After *Vicia Faba.*
1840.	14·0	26·5
		After *Brassica Rapa.*
1841.	11·0	15·2
		After *Vicia sativa.*
1842.	22·0	26·5
		After *Hordeum sativum.*
1843.	13·4	12·0
		After *Avena sativa.*
1844.	8·65	13·5
	Average of nine years . . 18·2	Average of nine years . . 18·7
	Average of first five years . 21·9	Average of first five years . 20·3
	Average of last four years . 13·7	Average of last four years . 16·8
	Maximum in one year . . 38·0	Maximum in one year . . 27·0
	Minimum in one year . . 11·0	Minimum in one year . . 11·0

The annexed diagram shows the variations in the yearly produce:

POPPIES.

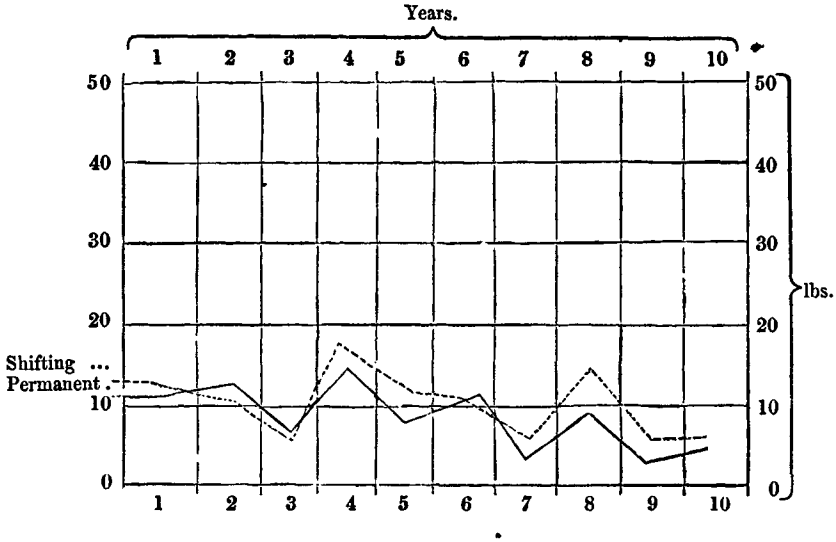


9. *Polygonum fagopyrum.*

Buckwheat, not being an exhausting crop, does not vary more than can be explained by differences of seasons and other contingent causes, the results being as follow :—

Year.	Permanent crop. Produce in a dried state.	Shifting crop. Produce in a dried state.
	No. 25.	
1835.	12·8	14·8
		After *Mentha viridis.*
1836.	14·0	11·5
		After *Valeriana Phu.*
1837.	6·0	5·7
		After *Papaver somniferum.*
1838.	16·0	18·0
		After *Vicia Faba.*
1839.	9·25	12·5
		After *Hordeum sativum.*
1840.	11·5	11·5
		After *Nicotiana rustica.*
1841.	3·2	5·5
		After *Nicotiana rustica.*
1842.	9·6	14·1
		After *Beta vulgaris.*
1843.	3·8	6·15
		After *Beta vulgaris.*
1844.	5·1	6·6
	Average of ten years . . . 9·1	Average of ten years . . . 10·6
	Average of first five years . 11·6	Average of first five years . 12·5
	Average of last five years . 8·7	Average of last five years . 8·7
	Maximum in one year . . 16·0	Maximum in one year . . 18·0
	Minimum in one year . . 3·2	Minimum in one year . . 5·7

BUCKWHEAT.



10. *Trifolium pratense.*

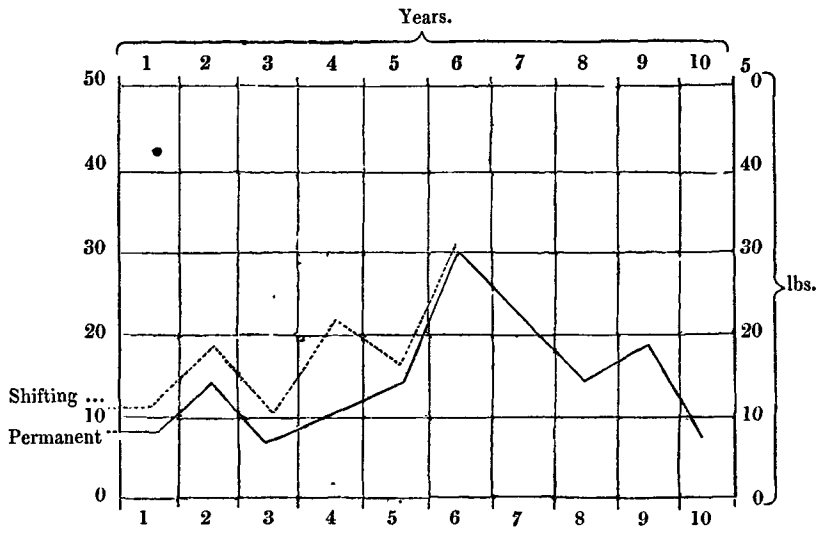
Red clover being a biennial, it was necessary to retain the shifting crop in the same bed for two years; and as the produce of the second year was in general larger than that of the first, the results can best be compared by stating the sum of each two years' growth.

Year.	Permanent crop. Produce of two years.	Shifting crop. Produce of two years.
	No. 12.	
1835. 8·5	22·7	11·3 28·3
1836. 14·2		17·0
		After *Mentha viridis.*
1837. 7·5	17·9	12·5 35·1
1838. 10·4		22·6
		After *Euphorbia lathyris.*
1839. 14·0	44·0	17·0 48·0
1840. 30·0		31·0
1841. 22·3	37·5	
1842. 15·2		
1843. 18·8	26·3	
1844. 7·5		

During the last four years it was not found convenient to introduce a shifting crop of clover.

The following curve represents the variations of the crop in the two cases.

CLOVER.



11. *Avena sativa.*

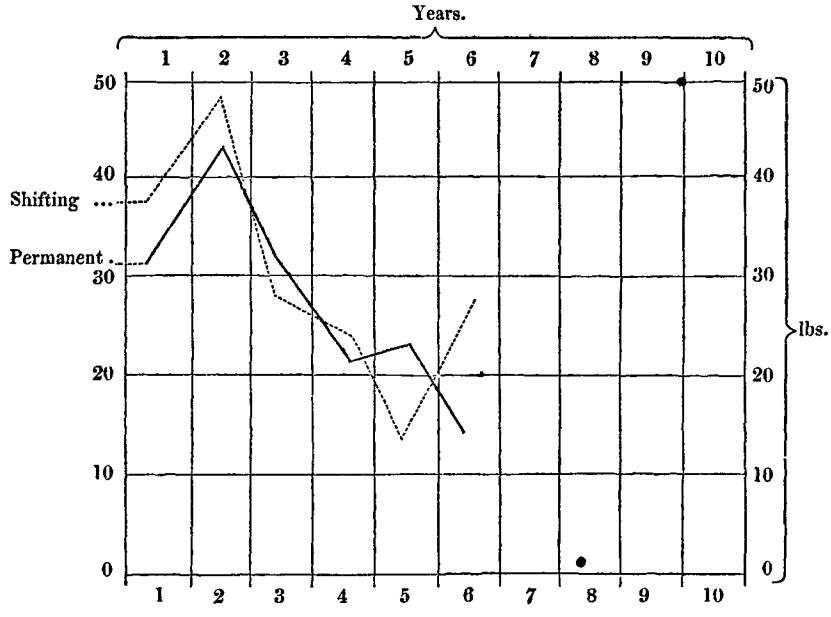
My trials with oats continued only for six years, and on ground already drawn upon by four crops since the period at which manure had been applied; the plot selected for the permanent crop having borne successively flax, beans, turnips, and hemp; that on which the corresponding crop in 1839 was raised, parsley, mint, and clover.

The produce therefore of the first year was in both cases moderate, and nearly uniform, but subsequently there was a greater diminution in the permanent than in the shifting crop, as will appear from the following Table:—

Year.	Permanent crop.	Shifting crop.
	No. 20.	After *Trifolium pratense.*
1839.	31·0	37·5
		After *Trifolium pratense.*
1840.	44·0	49·0
		After *Beta vulgaris.* | After *Trifolium pratense.*
1841.	31·7	53·0 | 28·5
		After *Polygonum fagopyrum.*
1842.	22·5	24·4
		After *Helianthus annuus.*
1843.	24·4	14·7
		After *Vicia sativa.*
1844.	14·6	28·3
	Average of six years . . . 28·0	Average of six years . . . 32·4
	Maximum in one year . . 44·0	Maximum in one year . . 49·0
	Minimum in one year . . 14·6	Minimum in one year . . 14·7

The following curve will show the variations in the yearly produce.

OATS.



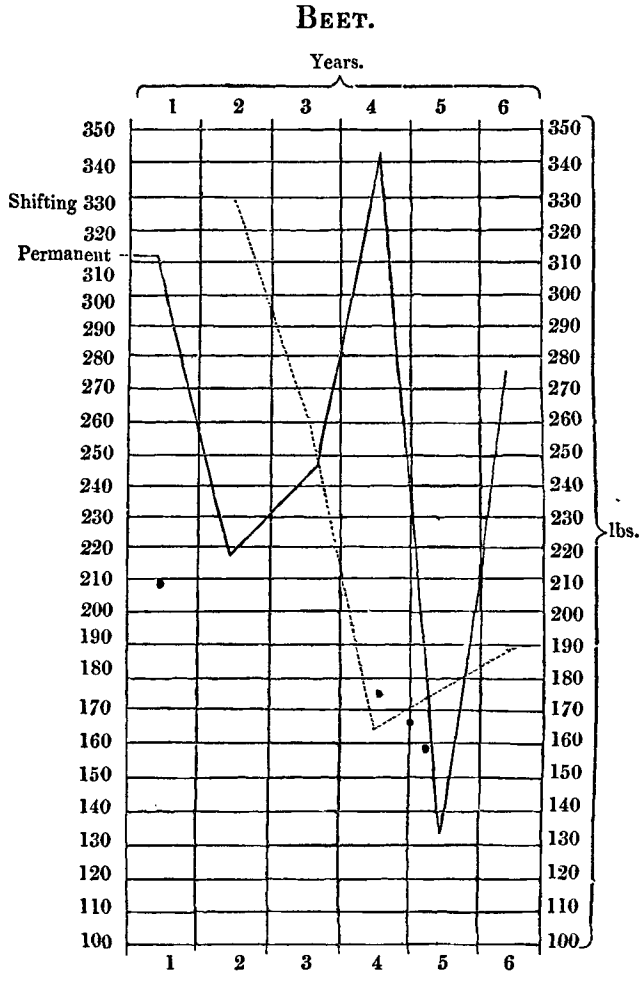
12. *Beta vulgaris.*

In the case also of the beet, the length of time during which the ground was cropped seems insufficient to lead to any decisive results, especially as the matters extracted from the soil by this plant are, compared to its bulk, inconsiderable.

The average of the shifting and permanent crops, it will be seen, does not vary materially, and what difference there is, seems in favour of the latter.

Year.	Permanent crop. Weighed in a green state.	Shifting crop. Weighed in a green state.
	No. 11 *b.*—After *Papaver somniferum.*	
1839.	312·0	None.
		After *Cannabis sativa.*
1840.	217·0	330·0
		After *Hordeum sativum.*
1841.	250·0	264·0
		After *Avena sativa.*
1842.	344·0	173·0
1843.	135·0	187·0
1844.	278·0	200·0

The following diagram will show the variations in the annual produce:—



13. *Cichorium endivia.* 14. *Mentha viridis.* 15. *Apium petroselinum.*

The three remaining crops, of Endive, Mint, and Parsley, were introduced into the series, from a wish to have one representative at least of the principal of those natural families, which supply us, with either plants useful for field or garden purposes, or with any of the commoner weeds which intrude themselves into our fields, it being conceived, that some interesting and useful results might be obtained, by watching the effect of their root excretions on plants of the same or of a different tribe.

Had it not been with a view to this theory, I should hardly have thought it worth while to experiment upon plants, which appear to draw comparatively so little from the soil, as the three now alluded to.

The following, however, were the results obtained:—

Cichorium endivia.

Year.	Permanent crop. Dried in the sun.	Shifting crop. Dried in the sun.
	No. 15.	
1834.	73·5	None.
	No. 16.	After *Polygonum fagopyrum.*
1835.	68·5	68·0
	No. 15 and 16.	After *Nicotiana rustica.*
1836.	44·4	49·0
		After *Hordeum sativum.*
1837.	33·3	34·0
		After *Vicia Faba.*
1838.	50·0	46·2
		After *Hordeum sativum.*
1839.	48·5	38·0
1840.	22·0	None.
1841.	58·5	None.
1842.	67·0	None.
1843.	50·0	None.
1844.	72·0	None.
	Average of ten years . . . 51·5	Average of five years . . 47·0
	Average of first five years . 53·9	Maximum of one year . . 68·0
	Average of last five years . 49·2	Minimum of one year . . 34·0
	Maximum in one year . . 73·0	
	Minimum in one year . . 22·0	

Mentha viridis.

Year.	Permanent crop. Produce in a dry state.	Shifting crop. Produce in a dry state.
	No. 21.	
1835.	21·9	17·0
		After *Apium petroselinum.*
1836.	53·0	26·0
		After *Apium petroselinum.*
1837.	38·0	9·25
1838.	18·3	36·8
1839.	26·75	None.
1840.	24·5	None.
1841.	16·8	None.
1842.	18·7	None.
1843.	12·8	None.
1844.	16·5	None.
	Average of ten years . . . 24·7	
	Average of first five years . 31·5	
	Average of last five years . 17·8	
	Maximum in one year . . 53·0	
	Minimum in one year . . 12·8	

Apium petroselinum.

Year.	Permanent crop. Produce in a dried state.	Shifting crop.
	No. 27.	
1835.	64·25	74·0
		After *Vicia Faba.*
1836.	115·0	208·0
		After *Polygonum fagopyrum.*
1837.	52·0	61·0
		After *Polygonum fagopyrum.*
1838.	58·0	68·5
		After *Mentha viridis.*
1839.	13·0	32·5
		After *Mentha viridis.*
1840.	59·0	83·0
1841.	22·7	None.
1842.	9·4	None.
1843.	12·0	None.
1844.	33·0	None.
	Average of ten years . . 39·75	Average of six years . . . 87·8
	Average of first six years . 60·2	Maximum in one year . . 208·0
	Average of last four years . 19·3	Minimum in one year . . 32·0
	Maximum in one year . . 115·0	
	Minimum in one year . . 9·4	

Tabular View of Experiments on the Rotation of Crops.

Name of the crop.	Whether planted in the same, or in different plots of ground.	No. of years during which the experiments were continued.	Amount of produce the first year.	Amount of produce the last year.	Highest amount of produce obtained during the whole period.	Year in which the highest amount of produce was obtained.	Smallest amount of produce obtained during the whole period.	Year in which the smallest amount of produce was obtained.	Average of the first five years.	Average of the whole period.	Crop obtained in 1844 from an equal space of ground previously well-manured.	General remarks.
			Tubers.	Tubers.	Tubers.		Tubers.		Tubers.	Tubers.	Tubers.	
Potatoes (*Solanum tuberosum*) .....1.	in the same	9	89·50	61·0	89·50	1st year, viz. 1836	48·6	8th year, viz. 1843	72·9	68·9	88·5	Merely cleaned from dust before weighing.
2.	in different	9	84·0	78·0	132·0	4th year, 1839, after Buckwheat.	48·4	8th year, 1843, after Vetches	92·8	89·1	.....	
3.	in the same	5	96·0	57·75	96·0	1st year, 1840	58·6	4th year, 1843.....	74·77	.....	.....	
Flax (*Linum usitatissimum*) .....1.	in the same	10	12·9	5·2	20·0	4th year, 1838	5·2	10th year, 1844	15·0	12·6	20·5	Dried in the sun before weighing.
2.	in different	10	15·8	17·8	34·0	8th year, 1842, after Turnips.....	15·8	1st year, 1835.....	19·9	22·7	.....	
3.	in the same	5	43·5	11·5	43·5	1st year, 1840.....	11·5	5th year, 1844.....	32·5	.....	.....	
Beans (*Vicia Faba*).....,.....1.	in the same	10	38·0	9·2	40·0	2nd year, 1836	9·2	10th year, 1844	32·8	24·7	25·0	Dried in the sun before weighing.
2.	in different	10	27·7	24·0	56·0	7th year, 1841, after Buckwheat.	24·0	10th year, 1844, after Tobacco	34·8	33·6	.....	
Barley (*Hordeum vulgare*) .....1.	in the same	10	29·0	28·7	42·5	4th year, 1838	21·5	3rd year, 1837.....	30·0	28·9	36·4	Dried in the sun before weighing.
2.	in different	10	35·5	30·5	75·0	4th year, 1838, after Endive.....	27·4	7th year, 1841, after Beans...	46·5	42·1	.....	
Turnips (*Brassica Rapa*) .....1.	in the same	10	185·0	77·0	185·0	1st year, 1835.....	37·2	3rd year, 1837.....	104·0	100·8	140·0	In a green state when weighed.
2.	in different	10	192 0	148·0	245·0	7th year, 1841, after Poppies ...	110·0	9th year, 1843, after Potatoes	173·0	176·5	.....	
Hemp (*Cannabis sativa*).....1.	in the same	9	46·5	21·5	46·5	1st year, 1836.....	20·5	5th year, 1840.....	32·55	30·13	42·0	Dried in the sun before weighing.
2.	in different	7	52·5	37·6	53·0	7th year, 1842, after Beet	22·5	2nd year, 1837, after Turnips	.....	40·0	.....	
Spurge (*Euphorbia lathyris*) .....1.	in the same	3	17·2	0·	.....	.....	.....	.....	.....	.....	.....	Dried before weighing.
Poppy (*Papaver somniferum*) .....1.	in the same	9	38·0	8·65	38·0	1st year, 1836.....	8·65	9th year, 1844.....	21·9	18·2	.....	Dried in the sun before weighing.
2.	in different	9	27·0	13·5	27·0	1st year, 1836, after Spurge	11·0	3rd year, 1837, after Endive	20·3	18·7	.....	
Clover (*Trifolium pratense*) .....1.	in the same	10	8·5	7·6	30·0	7th year, 1840.....	7·5	11th year, 1844	10·9	14·8	.....	Dried in the sun before weighing.
2.	in different	6	11·3	31·0	31·0	6th year, 1840, after Spurge	11·3	1st year, 1835, after Larkspur	.....	16·9	.....	
Endive (*Cichorium endivia*) .....1.	in the same	10	68·5	72·0	72·0	1st year, 1834.....	22·0	7th year, 1840	53·9	51·5	.....	Dried in the sun before weighing.
2.	in different	5	68·0	38·0	68·0	2nd year, 1835, after Buckwheat.	34·0	4th year, 1837, after Barley .	.....	47·0	.....	
Mint (*Mentha viridis*) .....1.	in the same	10	21·9	16·5	53·0	2nd year, 1836	12·0	9th year, 1843.....	31·5	24·7	.....	Dried in the sun before weighing.
2.	in different	4	17·0	36·8	36·8	4th year, 1838.....	9·25	3rd year, 1837.....	.....	22·5	.....	
Buckwheat (*Polygonum fagopyrum*) 1.	in the same	10	12·8	5·1	16·0	4th year, 1838.....	3·2	7th year, 1841.....	11·6	9·1	.....	Dried in the sun before weighing.
2.	in different	10	14·8	6·6	18·0	4th year, 1838, after Poppies.....	5·5	3rd year, 1837, after Valerian	12·5	10·6	.....	
Parsley (*Petroselinum sativum*).....1.	in the same	10	68·25	33·0	115·0	2nd year, 1836	9·4	8th year, 1842.....	61·25	39·75	.....	In a green state when weighed.
2.	in different	6	74·0	83·0	208·0	2nd year, 1836, after Beans	32·5	5th year, 1839, after Mint...	.....	87·8	.....	
Tobacco (*Nicotiana rustica*) .....1.	in the same	10	172·0	37·8	172·0	1st year, 1834.....	17·25	6th year, 1839.....	69·2	48·0	.....	Dried in the sun before weighing.
2.	in different	8	42·0	49·0	49·0	10th year, 1844, after Turnips ...	22·5	6th year, 1839, after Flax ...	.....	32·0*	.....	
Oats (*Avena sativa*) .....1.	in the same	6	31·0	14·6	44·0	2nd year, 1840	14·6	6th year, 1844.....	.....	28·0	.....	Dried in the sun before weighing.
2.	in different	6	37·5	14·7	53·0	2nd year, 1840, after Clover	14·7	5th year, 1843, after Sunflower	.....	32·4	.....	
Beet (*Beta vulgaris*) .....1.	in the same	6	312·0	135·0	344·0	4th year, 1842.....	135·0	5th year, 1843.....	.....	256·0	.....	In a green state when weighed.
2.	in different	5	330·0	200·0	330·0	1st year, 1840, after Hemp	173·0	3rd year, 1842, after Oats ...	.....	231·0	.....	
Tares (*Vicia sativa*) .....1.	in different	4	14·0	10·0	20·0	4th year, 1842.....	9·0	5th year, 1843.....	.....	13·25	.....	Dried in the sun before weighing.
Sunflower (*Helianthus annuus*) .....1.	in different	4	40·0	165·0	.....	.....	.....	.....	.....	.....	.....	Dried in the sun before weighing.

* The average is here lower than in the former, because two crops of a different kind had been previously taken off the ground before the first crop of Tobacco was sown. Hence this is so much inferior to the first crop of the preceding column.

It appears then that out of the whole series, there are only four cases in which the average amount of the permanent crop was equal or superior to that of the shifting one.

In the first of these, the Tobacco, the fact may be accounted for, from the condition of the ground being more favourable to the permanent than to the shifting crop on the year of its introduction, the former being obtained from soil which had been recently manured, the latter from what had been partially exhausted by preceding crops.

The second, the Beet, was scarcely continued for a sufficient length of time to lead to any certain conclusions.

The two others, namely, the Endive and Mint, present results so nearly agreeing in the amount of their permanent and shifting crops, that the slight disparity may be fairly referred to contingent circumstances, and an uniformity in the products obtained may in consequence be inferred.

Setting aside then the above four cases as exceptional, the general tenor of the experiments would seem to indicate a *manifest* advantage on the side of the shifting crops, varying from 1 to 75 per cent., but more generally approaching to the latter.

Yet it by no means follows that this difference is to be attributed to the influence of root excretions. Were such the cause, we ought to perceive a more regular, as well as a more rapid, diminution in the permanent crop than is indicated in these Tables; we should not find, for instance, the crop of potatoes equalling in the fifth year the produce of the first; the Turnips, after sinking to 37·0 lbs. in the third year, rising in the sixth to 128 lbs.; not to allude to other similar instances of oscillation.

If De Candolle's theory too could be carried out, we might have expected to find a more manifest improvement in the shifting crop occasionally occurring, owing to the excretions of the family of plants which had preceded it proving congenial to its constitution.

But if nothing positively injurious be imparted to the soil by the crop, the gradual falling off in the amount of the latter can only be attributed to the deficiency, either of organic, or of inorganic matter fitted for its development, in the soil in which it was reared.

Of the two continental writers on chemical agriculture whose works have excited the greatest interest in this country, the one would seem to favour the former, the other the latter explanation, although it may be more correct to consider them, as viewing the subject under two different aspects, rather than as laying down principles irreconcileable one with the other.

Liebig, for instance, although he regards the presence of certain inorganic matters as the only condition *essential* to the existence of a plant, does not deny, that its growth may be accelerated in proportion to the ready access to it of ammonia and carbonic acid, and these, it is evident, would be supplied more abundantly by the presence in the soil of organic matter in a readily decomposable condition.

Nor, on the other hand, would Boussingault deny the necessity for a supply of the

inorganic principles, which form, as it were, the skeleton of each plant, although he attributes the peculiar benefit derived from fallow crops to their power of generating the organic matter which is required by the cereals that are to succeed them.

In order to determine then in what degree the falling off of the permanent crop arose from the one or the other of these causes, it seemed necessary to obtain an analysis of the plants derived from this and from the shifting crop corresponding, and to compare the composition of both with that of a standard specimen of the same plant determined by the method pursued with respect to the two former; and it would have been also satisfactory, not only to ascertain, whether the soil itself originally contained such a store of all the principles existing in the crop, as might be sufficient to meet the demand made upon it for that purpose during the whole decennial period, but also whether its present composition was such, as actually indicated a deficiency in any of the principles which entered into the constitution of the plants grown in it.

It is evident that the former branch of the inquiry would have been superfluous, if I could have depended on two things:—

1st. That the analyses given by SPRENGEL and others, of the plants to which the inquiry related, were trustworthy; and

2ndly. That the composition of the same vegetable was at all times uniform both as to the quality and quantity of its ingredients.

But with respect to the former point, I found, on turning to the analyses given of the ashes of the same plants by different authorities, many marked discrepancies, and that those of SPRENGEL, which are the most numerous of any we could appeal to, were regarded as inaccurate by other chemists of higher distinction.

Nor, even if they had represented truly the composition of the plants which were actually examined by that analyst, could we be sure, that they would apply to those of the same species, grown in a different country, and under altered circumstances, more particularly as the recent researches of LIEBIG, WILL, FRESENIUS and others, appeared to indicate, that certain ingredients admit of being substituted for others, according to laws as yet not fully made out.

For all these reasons then, it became necessary for my purpose to obtain a correct analysis, both of the crops, and of the soil; and I was the more reconciled to the expenditure of labour involved in this undertaking, when I reflected, that the results obtained were likely not only to lead to an explanation of the cause of the utility of a rotation of crops, but also to throw some incidental light upon certain other points connected with the chemistry of agriculture, which did not appear to be sufficiently elucidated; such for instance, as the degree of variation of which a plant may admit in the quality and quantity of its inorganic ingredients, or in other words, its power of substituting one principle for another, and likewise as to the state of combination, in which the alkalies, phosphates, &c. exist with the other constituents of the soil, when in a condition to be assimilated by a plant.

I shall, therefore, next proceed to state the results of the analyses of the several crops which were made in my laboratory by Mr. WAY.

PART II.

On the chemical composition of certain crops cultivated in the Botanic Garden, and on the amount of inorganic principles abstracted by them from the soil during the period the experiments were continued.

It is only within a few years that the importance of ash analyses has been understood, and we were consequently much at a loss for accurate instructions as to the best method of conducting it.

A valuable paper has however recently appeared in the Memoirs of the Chemical Society of London (Part IX.), by WILL and FRESENIUS, which in a great degree supplies this deficiency, and which we therefore determined to adopt as the basis of our scheme of operations.

One part of it, however, relating to the determination of the phosphoric acid, was soon found extremely troublesome in practice, and too tedious to be resorted to in an inquiry which involved the necessity of so large a number of analyses. In this part therefore of the process, Mr. WAY suggested a method, which, as it recommended itself from its greater simplicity, and appeared to answer well in practice, he has adopted in all the cases, of which mention will hereafter be made.

But although the plan of analysis pursued presents in other respects but little of novelty, yet as certain modifications of the scheme of the German chemists have been here and there introduced, and as some of the manipulations may admit of being more clearly explained than in the paper alluded to, it will not be amiss to set down, as briefly as possible, all the principal steps pursued for the determination of the several ingredients existing in the ash.

In a few instances, as in the Cerealia, where the ashes abounded in silicates, complete solution in acids could not be effected, until the whole had undergone, either a previous fusion with carbonate of barytes, or evaporation with caustic potass, the former substance being employed for that portion of the ash which was to be examined for alkalies, the latter for the one set apart to ascertain the other ingredients.

But where the whole of the ash proved soluble in muriatic acid, no such preliminary process was required, and we were able to proceed directly to dissolve it in this menstruum.

A certain amount, however, of sand derived from the soil in which they had grown, and of charcoal, from the organic matter of the plant which had not been burnt off, was always present, and these of course would not be acted upon by this acid.

There was also in every instance a variable quantity of peroxide of iron proceeding manifestly from the vessels in which the combustion had been carried on, the quantity

to be burnt being too considerable to allow of its calcination in any of the platina vessels which I chanced to possess.

The ashes, therefore, of which 200 grains were usually taken, had first to be treated with pure muriatic acid, and the latter to be driven off by heat, so that the silica of the ash might be rendered insoluble.

Water and muriatic acid were then added to the dry mass, and the portion which did not dissolve was separated by filtration. Its weight, after being washed, represented the amount of silica in the ash, together with that of the extraneous sand and charcoal intermixed with it. The former was separated by digestion in pure dilute alkaline ley, and its quantity determined, in the first place indirectly, by the loss of weight sustained by the insoluble portion after its removal, and in the second more accurately, by the direct process of separating it from its solution in the alkali by treatment with an acid, and subsequent evaporation to dryness, after which, water having been added in sufficient quantity to redissolve the alkaline salt, the silica was collected on a filter, and then dried and ignited previously to weighing it.

The solution in dilute muriatic acid was made up to some definite quantity, so that it might be divided into four exactly equal portions, of which one was kept in reserve in case of any accident happening to the remainder, whilst the three others, which we will call A, B and C, were examined for the different ingredients present, as for instance,—

A. For the peroxide of iron.
B. For the phosphoric acid.
C. For the alkalies.

In most of the parts of vegetables, especially in their seeds, and in the tubers and bulbs which afford nutriment to animals, the amount of phosphoric acid may be expected to exceed that necessary for combining with the iron present. A reagent then which throws down phosphate of iron, affords us in these cases a ready means of estimating the whole amount of that metal, from the weight of phosphate obtained, and Will assures us* that 100 grains of the latter precipitate consists of 43·92 phosphoric acid, and 56·08 peroxide of iron.

When therefore, to the muriatic solution A, containing a slight excess of acid, acetate of ammonia is added, the muriatic acid, which had held the phosphate of iron in solution, is seized upon by the ammonia of the former salt, and the phosphate of iron, being insoluble in the liberated acetic acid, is precipitated.

We thus obtain a means of readily estimating the amount of peroxide of iron, but not of determining that of phosphoric acid, because there may be still a portion of the latter remaining in the liquid in combination with other bases, the phosphate of lime and of magnesia being soluble in free acetic acid, and the alkaline phosphates being so even in water.

In order therefore to estimate the amount of phosphoric acid, an expedient was

* Memoirs of the Chemical Society, part 9.

adopted, by which the presence of sufficient iron to carry down the whole of the phosphoric acid might be secured.

For this purpose a certain known weight of clean iron wire was dissolved in a mixture of nitric and muriatic acids, care being taken that no loss should occur from the violence of the action occasioned.

The solution thus prepared will then contain a definite amount of peroxide of iron, which, when introduced into the liquid containing the ash, will seize upon all the phosphoric acid, not already combined with iron, which it may contain.

Accordingly, after adding it to the latter, from which the phosphate of iron originally present had been previously thrown down by acetate of ammonia, we recover the whole of the metal, whether in combination with phosphoric acid or not, by applying again this same reagent, provided only the solution be rendered neutral by ammonia, and raised to a boiling temperature.

In the case supposed, therefore, the precipitate will indicate the whole of the phosphoric acid existing in the fluid, after the phosphate of iron originally present had been thrown down, together with the peroxide of iron which results from the iron introduced into it in union with chlorine.

Accordingly the quantity of phosphoric acid remaining after the first operation may be estimated, by deducting the weight of peroxide of iron, which is known, from that of the entire precipitate collected.

In practice however it was found most convenient to determine the amount of phosphoric acid, by taking another measured portion of the solution, viz. B, and adding to it in the first instance the known weight of iron. We are thus enabled, by following the steps above pointed out, to throw down, all the iron originally present in combination with phosphoric acid, all the phosphoric acid which may have existed in combination with other bases, and the whole of the peroxide of iron, whether proceeding from the ash, or introduced from without.

The amount of the former portion of the iron will have been ascertained by the examination of the solution A, whilst that of the latter can be readily calculated, as we know the weight of the iron introduced; by deducting therefore the sum of these two, which represents the total amount of peroxide of iron, from the entire weight of the precipitate, we obtain that of the phosphoric acid present in the ash.

This modification of the process saves some trouble, as it obviates the necessity of reducing the bulk of the solution remaining after the separation of the phosphate of iron precipitated from A. in the first process, which, owing to the number of washings necessary, becomes inconveniently large.

I felt curious to ascertain whether the phosphoric acid obtained by the above method was combined with two or with three atoms of base, as WILL and FRESENIUS state, that the Cerealia generally present it in the former predicament, the Leguminosæ in the latter.

Our experiments on this point do not appear to confirm such a conclusion, showing

rather, that the proportion of base to acid has some reference to the quantity of alkali present, and is therefore dependent in a certain degree upon the manner in which the previous calcination had been conducted.

Supposing a plant to be rich in alkali, and to contain but little silex, it would seem natural to expect, that the phosphoric acid would be united with three atoms of bases.

When, on the contrary, the proportion of silica was large, a strong heat would cause more of it to unite with the alkali, and hence there might be only enough of the latter remaining to form a bibasic combination with phosphoric acid. Yet even here, if a slighter heat had been applied, it might happen that a tribasic compound would be produced.

Thus we found, that in all the three crops of potatoes the phosphate gave a yellow precipitate with nitrate of silver, and the same was also the case in the turnips; but in only one sample of beans, viz. the shifting crop, and in one of barley, which was also the shifting one, did the same hold good.

On the other hand, in two other samples of ash from the barley, and in two samples of that from the beans, the phosphoric acid seemed, from the precipitate afforded by nitrate of silver, to be united with two atoms only of base.

It is easy to determine the amount of lime and of magnesia from either of the liquids already operated upon, oxalate of ammonia being added to separate the former, and, after neutralizing with ammonia the acid solution, phosphate of soda throwing down the magnesia.

In these respects the common methods were adhered to.

The solution C. was reserved for the determination of the alkalies.

For this purpose it is necessary to get rid of all the earths and metallic oxides which may be present, which is accomplished by adding barytic water so long as a precipitation takes place. That reagent of course throws down the whole of the sulphuric and phosphoric acids, the peroxide of iron, most of the magnesia, and most of the lime*.

The filtered solution may however contain a little magnesia and lime, and probably much barytes.

To remove these, carbonate of ammonia is added in excess, and the precipitate which is thrown down removed by filtration, after being allowed to stand until it becomes heavy and granular.

If this be duly performed, the remaining solution can contain only muriate of ammonia and chlorides of the fixed alkalies.

The former is removed by heat, and the dry chlorides then remaining will repre-

* As the entire precipitate, excepting what consists of sulphate of barytes, is soluble in muriatic acid, we may estimate the amount of sulphuric acid present, by treating it with the former acid, removing all that is soluble in water by filtration, and lastly weighing the dried residue, from which the weight of sulphuric acid may be readily deduced.

sent the weight of the alkaline salts originally present in the ash. Having ascertained this, the dry residue is dissolved in a small quantity of water, chloride of platinum added, and the whole evaporated nearly to dryness. It is then treated with dilute alcohol, which takes up the double chloride of platinum and sodium*, together with any excess of the reagent that may have been added. The undissolved residue is the double chloride of platina and potass, from which the amount of the chloride of potassium may be calculated. The difference between the weight of the latter and of the whole salt gives that of the chloride of sodium proceeding from the ash.

The carbonic acid present was best ascertained by operating on a separate portion of the ash, and the common method of determining it by the loss of weight consequent upon the addition of a stronger acid was adopted, with the precautions usually taken†.

The chlorine also was determined in the usual manner by nitrate of silver, a separate portion of the ash being employed for that purpose.

In the analyses given, it has been usual to consider it as in combination either with sodium or with potassium. That this was the case, seemed probable from the curious relation generally found to subsist between the quantity of chlorine and of sodium detected, which in many instances approximated so nearly, that we were led to conclude, that the chlorine in these instances merely implied a corresponding amount of chloride of sodium existing in the ash. That the correspondence should not have been exact, may be more readily explained, when we consider that the only generally practicable mode of estimating soda is an indirect one, and therefore liable to some degree of uncertainty.

In the few instances where the amount of chlorine was more than proportionate to that of the sodium, it was thought consistent with analogy to regard that portion of the former which was in excess, as held in combination with the vegetable alkali, or as representing an equivalent weight of chloride of potassium.

This mode of stating the results may appear objectionable, as blending theory with fact, but my reason for adopting it is, that it points at an important general conclusion, which it is hoped future inquiries will either negative or confirm, namely that the base of the soda found in plants commonly enters them in a state of combination with chlorine, being derived from the common salt, taken up, but not decomposed, by the organs of the plant.

Such an inference indeed cannot be adopted by those who receive the analyses given by SPRENGEL as correct, for in many of these large quantities of soda are stated

* This double chloride is readily decomposed, if first rubbed up with mercury, which flies off along with the chlorine in the form of calomel, when heated.

† It seems a defect in the analyses reported by SPRENGEL, that this ingredient is never mentioned in them; for although it may not be present as such in the crop, yet its amount in the ash probably represents that of the organic acids existing in the plant previously to its being burnt, and hence the proportion which it bears in different samples of the same species to the phosphoric and other mineral acids, may tend to indicate the relation subsisting between the amount of organic and of inorganic matter, arising from the mode of culture or other circumstances.

as having been detected; but, without presuming to bring forward the analyses made in my laboratory as in themselves sufficient to justify the public in rejecting the former as inaccurate, I may be permitted to observe, that it is much more easy to conceive that the amount of soda present may have been *overrated*, than that it should have been estimated *below* its real amount, supposing anything like an equality of skill and attention on the part of the operator.

To overrate it, we need only attribute to him some degree of negligence, either in not converting by means of chloride of platinum the whole amount of chloride of potassium into the sparingly soluble double chloride, or in not determining its entire quantity; to estimate it too low, we must imagine, what is far less probable, a portion of the readily soluble compound of chlorine with sodium, or the equally soluble double salt which the latter forms with platinum, to remain attached to the chloride of potassium and platinum, and thus to add to its weight.

Our results may also appear to militate against the conclusions of a much higher authority than SPRENGEL, I mean Professor LIEBIG, who has lately represented that one alkali may be substituted for another in the organization of a plant, and that a species, which in inland spots assimilates a certain amount of potass, takes into its frame an equivalent proportion of soda in maritime districts, where the latter alkali abounds.

With the slender data before me, it would be the height of presumption to impugn the generalisations of this distinguished philosopher, but it will be seen from the analyses given below, that no difference in the nature of the alkaline ingredients could be detected between barley, taken from the neighbourhood of the sea, whether from the eastern or western coasts of this country, and from the more central region of Oxfordshire.

Two ingredients mentioned by SPRENGEL as existing in the ashes of plants were searched for in a few of those to which this paper refers, but without success. These were alumina and manganese, the former so universally present in the soil, that it may readily find admission into the ashes of the plants, unless the greatest care be taken to clean off every particle of dirt entangled by their roots; the latter, as LIEBIG thinks, an accidental ingredient, being taken up by many plants in considerable quantities where the soil contains much of it, but altogether wanting in the same vegetables cultivated elsewhere.

In order to ascertain the presence of alumina, the ash was dissolved in muriatic acid, the solution evaporated to dryness, in order to separate the silica, and then redissolved in muriatic acid diluted with water.

An excess of ammonia was afterwards added to the filtered liquor, and the precipitate which fell, after having been well-washed, was boiled with a pure solution of potass. The portion dissolved was then filtered, neutralized with muriatic acid, and treated a second time with ammonia. If any precipitate had been thrown down, the presumption would have been that it consisted of alumina, and the appropriate tests would have been applied to confirm the conjecture; but in the only instance in which

we could positively assure ourselves that no admixture of the soil had got in, namely, in the grain of barley from Ensham, nothing was thrown down by the last application of ammonia, and in one sample of ash from flax (viz. the standard crop), only a mere trace was discoverable.

Considering indeed that the soluble salts of alumina are poisonous to plants, and that the earth itself is confessedly present in very variable, sometimes very minute, quantities, I am inclined to doubt whether it be in reality a constituent of their ashes at all.

With respect to manganese, two methods were adopted for ascertaining its presence.

The first, that of boiling the muriatic solution with carbonate of lime, and then, after filtering it, adding hydrosulphuret of ammonia.

The second, the blow-pipe test, fusing a little of the muriatic salt with borax, when a very minute quantity of manganese would produce its characteristic colour in the bead of glass produced.

By neither of these methods were any indications of manganese obtained.

I next proceed to state the results of the analyses made in my laboratory by Mr. Way, of six kinds of crops grown in the experimental garden, together with those obtained from certain standard crops of the same species, grown in another part of the garden, or in other places in the vicinity of Oxford, under more natural circumstances.

My original object being merely that of ascertaining the quality and quantity of the inorganic matters abstracted from the soil in these instances, the crop of barley, flax, hemp and beans, was burnt altogether without any separation of their respective parts having been previously made, and it was only in the case of the potatoes and the turnips that a distinct portion of the plant was selected for analysis, namely, the tubers in the former, and the bulbs in the latter.

Barley.

Permanent crop, after ten years' repetition.

100 grains of the dried crop, including both the straw and grain, left of ash 8·7 grains.

100 grains of this ash contained as follows:—

Sand and charcoal, extraneous	22·36
Peroxide of iron, chiefly extraneous	2·12
	24·48
Silica of the plant	24·60
Phosphoric acid	7·31
Sulphuric acid	2·12
Carbonic acid	1·94
Chloride of sodium	4·73
Potass	17·33
Magnesia	4·68
Lime	13·91
Total	101·14

Barley.

Grown in the same part of the garden as the last, for ten years unmanured, distinguished as the *shifting crop*.

100 grains of the dried crop, including as before both the straw and the grain, left of ash 6·25 grains.

100 grains of this ash contained as follows:—

Sand and charcoal, extraneous	21·91
Peroxide of iron, chiefly extraneous	2·30
	24·21
Silica of the plant	36·47
Phosphoric acid	9·30
Sulphuric acid	2·35
Carbonic acid	1·44
Chloride of sodium	1·43
Potass	16·58
Magnesia	3·58
Lime	7·72
	103·08

Barley.

Grown in a distinct part of the garden. Soil similar, but recently manured, distinguished as the *standard crop*.

100 grains of the dried crop, including as before the straw and grain, gave of ash 7·15 grains.

100 grains of this ash contained as follows:—

Sand and charcoal, extraneous	16·60
Peroxide of iron, chiefly extraneous	2·30
	18·90
Silica of the plant	37·27
Phosphoric acid	7·67
Sulphuric acid	4·37
Carbonic acid	1·51
Chloride of sodium	1·84
Potass	13·86
Magnesia	3·96
Lime	11·81
	101·19

It would appear then from the above analyses, that the principal difference between the permanent crop and the two others consisted in the larger amount of soluble silica, which, together with the greater proportion of ash, may have arisen from the straw predominating in quantity over the grain.

It next appeared to me desirable, both by way of testing the accuracy of these results, and likewise of ascertaining whether the third sample of barley analysed might really be adopted as a fair representative of a standard crop, to examine separately the grain and straw taken from a crop of average quality grown in the neighbourhood of Oxford.

Mr. DRUCE of Ensham accordingly supplied me with a sample of barley from his farm, of which the following analysis was made by Mr. WAY.

1000 parts of the crop of barley from a field near Ensham, situated on the Oxford clay, consisted of—

Grain	575
Aulm	37
Straw	388
	1000

Of the Grain.—100 parts yielded of ash 2·04 parts, 100 grains of which consisted of—

			Excluding extraneous matter.
Charcoal*, extraneous .	24·51	Silica	24·51 or 33·2
Peroxide of iron, extraneous	2·30	Phosphoric acid .	22·97 or 31·2
	26·81	Sulphuric acid . .	2·48 or 3·4
Ingredients of the grain .	73·86, viz.—	Carbonic acid . .	
		Chloride of sodium	1·48 or 2·3
		Potass	14·10 or 19·1
		Magnesia . . .	5·63 or 7·6
		Lime	2·71 or 3·6
Total . . .	100·69		73·86 100·4

Of the Straw.—100 parts yielded 4·2 of ash, of which 100 parts contained—

			Excluding extraneous matter.
Sand and charcoal, extraneous	4·20	Silica	44·72 or 47·20
Peroxide of iron, extraneous .	4·74	Phosphoric acid .	1·68 or 1·80
	8·94	Sulphuric acid .	4·38 or 4·60
Ingredients of the straw. .	94·62, viz.—	Carbonic acid .	1·21 or 1·27
		Chloride of sodium	7·85 or 8·25
		Soda	0·98 or 1·06
		Potass	22·98 or 24·40
		Magnesia . . .	1·67 or 1·70
		Lime	9·15 or 9·65
Total	103·56		103·56 99·93

* In this instance the extraneous matter not dissolved by muriatic acid proved to consist almost wholly of

Of the Aulm.—100 parts yielded 13·7 of ash, of which 100 parts contained—

				Excluding extraneous matter.
Charcoal and sand, extraneous	6·22	Silica	80·96	or 89·50
Peroxide of iron, extraneous	1·53	Phosphoric acid	1·20	or 1·30
	7·75	Sulphuric acid	0·89	or 0·90
Ingredients of the aulm	90·58, viz.—	Carbonic acid	a trace	0·00
Total	98·33	Chloride of sodium	0·73	or 0·80
		Soda	0·22	or 0·24
		Potass	1·23	or 1·30
		Magnesia	0·90	or 0·99
		Lime	4·50	or 5·00
			98·33	100·03

According to these data the crop of barley will consist as follows :—

	Parts.		Parts.		Parts.	Total.
Grain	57500	Straw	38800	Aulm	568	96868.
Yielding of ash	1170	Yielding of ash	1629	Yielding of ash	77·5	3406·5
Of which the extraneous matter amounted to	313	Of which the extraneous matter amounted to	141	Of which the extraneous matter amounted to	5·5	459·5
Real ash	857	Real ash	1488	Real ash	72·0	2417·0
The latter consisting of		Consisting of		Consisting of		
Silica	287	Silica	705	Silica	64·50	1056·50
Phosphoric acid	270	Phosphoric acid	26	Phosphoric acid	0·13	296·13
Sulphuric acid	29	Sulphuric acid	70	Sulphuric acid	0·65	99·65
Carbonic acid	00	Carbonic acid	18	Carbonic acid	0·00	18·00
Chloride of sodium	17	Chloride of sodium	122	Chloride of sodium	0·57	139·57
Soda	00	Soda	15	Soda	0·14	15·14
Potass	164	Potass	361	Potass	0·93	525·93
Magnesia	65	Magnesia	25	Magnesia	0·65	90·65
Lime	31	Lime	143	Lime	3·60	177·60
	863		1485		71·17	2419·17

Now according to this calculation, 100 parts of the mixed ash ought to contain the subjoined quantities of the ingredients below-mentioned, and by comparing these with the composition given of the ashes obtained from the three crops grown in the Botanic Garden, which I have deduced from the analysis before given, after deducting in each instance the matters regarded as extraneous, it will be seen that there is a near correspondence.

charcoal, for after the first analysis had been completed, another portion of the ash was fused with potass, after which the silica obtained agreed within 0·2 with that procured in the first instance by the usual process.

I am the more anxious to state this, as it will be seen from the statement given in a subsequent page, that there is a great discrepancy between the per-centage of silica given in Mr. WAY's analysis and that reported by SPRENGEL, a discrepancy which, without this explanation, might be attributed to a want of care on his part in not dissolving the whole of the silica.

100 grains of real ash contain,—

	Druce's Barley and Barley-straw.	Botanic Garden.		
		Permanent.	Shifting.	Standard.
Silica	43·6	32·3	46·1	45·0
Phosphoric acid	12·2	9·5	11·8	9·2
Sulphuric acid	4·1	2·7	2·9	5·2
Carbonic acid	0·7	2·5	1·7	1·8
Chloride of sodium	5·7	6·1	1·7	2·1
Soda	0·6	0·0	0·0	0·0
Potass	21·7	22·6	20·9	16·8
Magnesia	3·7	6·0	4·4	4·7
Lime	7·3	18·2	9·7	14·3
Total	99·6	99·9	99·2	99·1
Or Acids	17·00	14·7	16·4	16·2
Bases	32·70	46·8	35·0	35·8

These results are interesting on two accounts; first, as they show what the composition of barley is when cultivated under natural circumstances, or within what limits its variation from a normal condition may be circumscribed; secondly, as they confirm the general exactness of the preceding analyses, by the correspondence which is seen to exist between the composition of the shifting crop, as ascertained by experiment, and that of the sample obtained from Mr. Druce's brought out by the above method of computation.

As the analyses of the ash, both in the case of the straw and of the grain, were performed by Mr. Way, whilst the proportion between the grain and straw, as well as that subsisting in each instance between the crop and its ash, was ascertained by myself, the statement which I have just submitted as to the real composition of the crop, calculated from these data, would hardly have presented so near an accordance with the analysis made of the entire crop which I had obtained in the Botanic Garden, had not both the one and the other been executed with considerable care.

It was far otherwise, however, when we compared our results with those of Sprengel, in which, amongst other striking discrepancies, we observe, that the proportion of soda stated to exist in the grain exceeds that of the potass, whilst in our analyses, only so much as was equivalent to the amount of chlorine appeared to be present*.

* Our analysis of *Hordeum vulgare*.		Sprengel's, of *Hordeum distichum*.		Bichon's, as quoted by Will.	
Silica	33·2	Silica	50·2	Silica	21·99
Phosphoric acid	31·2	Phosphoric acid	8·4	Phosphoric acid	40·63
Sulphuric acid	3·4	Sulphuric acid	2·5	Sulphuric acid	0·26
Carbonic acid	0·0	Carbonic acid	0·0	Carbonic acid	0·00
Chloride of sodium	2·3	Chloride of sodium	0·0	Chloride of sodium	0·00
Potass	19·1	Potass	11·80	Potass	3·91
Soda	0·0	Soda	12·30	Soda	16·79
Magnesia	7·6	Magnesia	7·65	Magnesia	10·05
Lime	3·6	Lime	4·50	Lime	3·36
Alumina	0·0	Alumina	1·50	Alumina	0·00
	100·4		99·95		97·03

This discrepancy made me desirous of learning, whether, in accordance with the observations of Will and Fresenius, any marked variation in the character of the alkaline constituent might subsist between barley cultivated in an inland county like Oxfordshire, and near the sea, and I therefore procured, through the kindness of a friend, one sample from the coast of Essex, and another from that of Cardiganshire, in South Wales.

The following were the results obtained of barley from the sea coast of Essex :—

1000 grains yielded 19 grains of ash.

100 grains of this ash contains, of—

Sand and charcoal, extraneous	23·28	
Peroxide of iron, extraneous	2·44	
	25·72	
Silica	24·89, or, excluding extraneous matter	34·0
Phosphoric acid	21·84, or, excluding extraneous matter	29·7
Sulphuric acid	1·79, or, excluding extraneous matter	2·4
Carbonic acid	0·00	0·0
Chloride of sodium	0·00, there being no chlorine in the ash.	
Soda	1·05, or, excluding extraneous matter	1·3
Potass	15·42, or, excluding extraneous matter	21·1
Magnesia	5·29, or, excluding extraneous matter	7·2
Lime	3·36, or, excluding extraneous matter	4·5
	99·36	100·2
	25·72	
Real ingredients of plant	73·64	

Barley-straw belonging to the same crop from Essex.

1000 grains yielded 49 grains of ash.

100 grains of these ashes contain, of—

Sand and charcoal, extraneous	8·59	
Peroxide of iron, extraneous	4·26	
	12·85	
Silica	41·81, or, excluding extraneous matter	48·9
Phosphoric acid	4·18, or, excluding extraneous matter	4·9
Sulphuric acid	0·67, or, excluding extraneous matter	0·8
Carbonic acid	0·00	0·0
Chloride of sodium	9·58, or, excluding extraneous matter	11·2
Soda	0·65, or, excluding extraneous matter	0·7
Potass	18·49, or, excluding extraneous matter	21·6
Magnesia	4·95, or, excluding extraneous matter	5·7
Lime	5·18, or, excluding extraneous matter	6·1
	98·36	99·9
	12·85	
Real ingredients	85·51	

The straw of the barley grown on the coast of Wales was not examined, but the ash of the grain from that quarter was found to contain, in 100 parts, 9·64 of potass, 1·32 of chloride of sodium, and only 0·84 of soda, the smaller proportion of potass being explained by the larger amount of extraneous matter present in the residuum left by it after calcination, than by the samples previously noticed.

The small proportion of soda, however, both in this, and in the former case in which the sample was obtained from the neighbourhood of the sea, seems to militate against the general conclusion deduced by WILL, from his analysis of barley taken from the interior of Germany, as compared with the same brought from the Netherlands.

POTATOES.

The crop grown in a recently manured portion of the Botanic Garden, separate from the spot set apart for the experiments, proving defective in quality, I selected as my standard a good mealy sort reared in the neighbourhood of Oxford, in the same kind of subsoil. The following will give the relative composition of this, and of the two crops obtained from the ground left for ten years unmanured, which, in the case of that styled the permanent, had borne potatoes for ten years consecutively, whilst in that styled the shifting, it had only borne them in 1844, having been occupied with the following plants on the nine years preceding, viz.—

1835. Delphinium consolida.
1836. Trifolium pratense.
1837. Nicotiana rustica.
1838. Valeriana Phu.
1839. Valeriana Phu.
1840. Linum usitatissimum.
1841. Solanum tuberosum.
1842. Papaver somniferum.
1843. Hordeum sativum.

Standard Crop.

Of the tubers, 1000 grains yielded about . . .	7·6	of ashes.
	755·0	of water.
	236·4	solid organic matter.
	1000·0	

Overlooking the small amount of extraneous matter intermixed, the ash will of course represent the proportion which its inorganic constituents bear to the whole quantity.

Now 100 grains of this ash consisted of—

Sand and charcoal, extraneous . . .	5·93
Peroxide of iron, extraneous	6·85
	12·78
Silica	5·81
Phosphoric acid	9·68
Sulphuric acid	5·23
Carbonic acid	5·84
Chloride of sodium	2·06
Chloride of potassium	6·67
Potass	37·99
Magnesia	10·98
Lime	2·71
	99·75
	12·78
Real ingredients	86·97

Permanent Crop of Potatoes.

Tubers, 1000 grains yielded	12·7 of ash.
	724·0 of water.
	263·3 solid organic matter.
	1000·0

100 grains of this ash consisted of—

Sand and charcoal, extraneous	14·40
Peroxide of iron, extraneous	3·30
	17·70
Silica	1·57
Phosphoric acid	10·68
Sulphuric acid	3·74
Carbonic acid	10·68
Chloride of sodium	2·79
Chloride of potassium	3·09
Potass	37·47
Soda	0·00
Magnesia	7·00
Lime	3·64
	98·24
	17·70
Real ingredients	80·54

Shifting Crop of Potatoes.

Of the tubers, 1000 grains yielded	10·8 of ash.
	719·0 of water.
	270·2 solid organic matter.
	1000·0

100 grains of this ash consisted of—

Sand and charcoal	2·16
Peroxide of iron	5·15
	7·31
Silica	6·60
Phosphoric acid	15·13
Sulphuric acid	2·21
Carbonic acid	11·03
Chloride of sodium	1·87
Chloride of potassium	0·00
Potass	46·12
Soda	0·78
Magnesia	6·31
Lime	2·54
	99·88
	7·31
Real ingredients	92·57

The above analyses of potatoes, it may be observed, agree more nearly with BOUSSINGAULT than with SPRENGEL, as will appear by the following Table :—

Ingredients.	BOUSSINGAULT.	SPRENGEL.	WAY.		
			Permanent.	Shifting.	Standard.
Silica	5·6	1·0	1·95	7·150	6·67
Phosphoric acid . . .	11·3	4·8	13·30	16·200	11·15
Sulphuric acid . . .	7·1	6·5	4·66	2·370	6·00
Carbonic acid . . .	13·4	0·0	13·30	11·900	6·70
Chloride of sodium . .	0·0	0·0	3·43	1·950	2·30
Chloride of potassium .	0·0	0·0	0·00	0·000	7·60
Soda	traces.	28·5	0·00	0·840	0·00
Potass	51·5	48·2	46·60	50·00	43·80
Magnesia	5·4	3·9	8·70	6·85	12·65
Lime	1·8	4·0	4·54	2·70	3·10

The correspondence between the standard crop analysed by Mr. WAY and the one analysed by BOUSSINGAULT, is in many particulars exceedingly close ; there is indeed an excess of magnesia and some little deficiency of potass, but if the potassium present in 7·6 of chloride (which is equivalent to 4·0) be represented as potass, it will amount to 4·8, which, added to 43·8, brings up the proportion of potass to 48·6, or to more than that present in the permanent crop.

TURNIPS.

The next kind of crop which we analysed was the turnips, and the following were the results obtained :—

Standard sort from the neighbourhood of Oxford, contained about 10 of water, and 1 of organic matter.

1000 grains yielded 3·18 of ashes ; 100 grains of which consisted of—

Sand and charcoal	5·28
Peroxide of iron	11·91
	17·16
Silica	3·81
Phosphoric acid	12·63
Sulphuric acid	7·17
Carbonic acid	7·04
Chloride of sodium	4·83
Soda	2·57
Potass	31·62
Magnesia	3·18
Lime	11·54
	101·55
	17·16
Real ingredients	84·39

Turnips.

Shifting crop.

Sand and charcoal	4·01
Peroxide of iron	3·81
	7·82
Silica	3·30
Phosphoric acid	10·77
Sulphuric acid	9·43
Carbonic acid	8·66
Chloride of sodium	0·00
Chloride of potassium	5·40
Potass	38·46
Magnesia	5·08
Lime	10·44
	99·42
	7·82
Real ingredients	91·60

Turnips.

Permanent crop.

Sand and charcoal	3·92
Peroxide of iron	2·80
	6·72
Silica	2·67
Phosphoric acid	12·80
Sulphuric acid	11·07
Carbonic acid	9·75
Chloride of sodium	1·74
Soda	0·00
Potass	39·44
Magnesia	3·83
Lime	11·81
	99·73
	6·72
Real ingredients	93·01

Here also there is a pretty near coincidence between the analysis of Mr. Way and that of Boussingault, excepting in the amount of phosphoric acid, which corresponds nearly to that reported by Sprengel, with whom however in other respects there is but little agreement, as will appear from the following Table:—

Ingredients.	Boussingault.	Sprengel.	Way.		
			Permanent.	Shifting.	Standard.
Silica	6·4	7·8	2·87	3·60	4·5
Phosphoric acid	6·0	14·0	13·70	11·80	14·9
Sulphuric acid	10·9	7·9	11·80	10·30	8·5
Carbonic acid	0·0	0·0	10·40	9·40	8·3
Chloride of sodium	0·0	0·0	1·83	0·00	5·7
Chloride of potassium	0·0	0·0	0·00	5·90	0·0
Soda	4·1	21·0	0·00	0·00	3·1
Potass	33·7	14·0	42·40	42·00	37·4
Magnesia	4·3	4·3	4·10	5·60	3·8
Lime	10·9	24·4	12·81	11·30	13·6

	Boussingault.	Sprengel.	Way.		
			Permanent.	Shifting.	Standard.
Potass	33·7	21·0	42·40	45·74†	37·40
Soda	4·1	14·0	1·14*	0·00	6·16‡
Alkalies	37·8	35·0	43·54	45·74	43·56

Hemp.

Standard crop grown in the Botanic Garden apart from the portion reserved for the experiments.

100 grains of the crop left 6·1 of ashes, 100 grains of which contained of—

Charcoal and sand	7·48
Peroxide of iron	2·78
	10·26
Silica	5·58
Phosphoric acid	5·44
Sulphuric acid	1·09
Carbonic acid	19·81
Chloride of sodium	1·72
Soda	0·98
Potass	13·71
Magnesia	7·67
Lime	34·03
	100·29
	10·26
Real ingredients	90·03

* Viz. Ch. Sod. 1·83 = Soda 1·14.

† Viz. Potass ..... 42·00
Ch. Pot. 5·9 = Potass ..... 3·74
45·74

‡ Viz. Soda ..... 3·10
Ch. Sod. ..... 5·7=3·06
6·16

Hemp.

Shifting crop.

100 parts yielded 7·01 of ash, 100 parts of which consisted of—

Sand and charcoal	8·30
Peroxide of iron	3·78
	12·08
Silica	8·71
Phosphoric acid	5·68
Sulphuric acid	0·73
Carbonic acid	20·10
Chloride of sodium	0·63
Chloride of potassium	0·00
Soda	0·14
Potass	7·49
Magnesia	5·19
Lime	39·00
	99·75
	12·08
Real ingredients	87·67

Hemp.

Permanent crop.

100 parts yielded 6·00 of ash, 100 parts of which consisted of—

Sand and charcoal	10·40
Peroxide of iron	3·94
	14·34
Silica	8·39
Phosphoric acid	4·50
Sulphuric acid	1·09
Carbonic acid	19·78
Chloride of sodium	0·43
Soda	0·06
Potass	7·25
Magnesia	2·18
Lime	40·10
	98·12
	14·34
Real ingredients	83·78

The following will give a tabular view of the composition of the above three crops in 100 parts, after deducting the extraneous matters present in the ash.

Ingredients.	Standard.	Shifting.	Permanent.
Silica	6·13	9·95	10·00
Phosphoric acid. . . .	6·00	6·50	5·35
Sulphuric acid	2·00	0·83	1·20
Carbonic acid	21·79	23·00	23·50
Chloride of sodium . .	1·89	0·72	0·47
Soda	1·08	0·16	0·07
Potass	15·08	8·55	8·62
Magnesia	8·43	5·95	2·62
Lime	37·40	44·60	47·60
	99·80	100·26	100·00

Flax.

Standard crop grown in the Botanic Garden apart from the spot reserved for the experiments.

100 grains of the crop, including the ripened seeds, yielded 10·7 of ash, of which latter 100 parts contained—

Sand and charcoal	12·02
Peroxide of iron	3·42
	15·44
Silica	1·77
Phosphoric acid	6·85
Sulphuric acid	5·10
Carbonic acid	15·69
Chloride of sodium	2·43
Chloride of potassium	6·04
Soda	0·00
Potass	21·73
Magnesia	3·89
Lime	18·30
	97·24
	15·44
Real ingredients	81·80

Flax.

Shifting crop.

100 grains of the crop yielded 8·0 of ashes, 100 grains of which consisted of—

Sand and charcoal	13·15
Peroxide of iron	5·41
	18·56
Silica	1·82
Phosphoric acid	6·77
Sulphuric acid	4·18
Carbonic acid	17·38
Chloride of sodium	1·58
Soda	1·05
Potass	20·51
Magnesia	4·72
Lime	21·56
	98·13
	18·56
Real ingredients	79·57

Flax.

Permanent crop.

100 grains of the crop yielded 6·675 of ash, 100 grains of which consisted of—

Sand and charcoal	7·05
Peroxide of iron	8·01
	15·06
Silica	6·55
Phosphoric acid	6·55
Sulphuric acid	3·12
Carbonic acid	12·20
Chloride of sodium	1·14
Soda	5·87
Potass	11·05
Magnesia	4·68
Lime	33·59
	99·81
	15·06
Real ingredients	84·75

The following will show the composition of the three crops of flax, after deducting the extraneous matters present in the ash.

Ingredients.	Standard.	Shifting.	Permanent.
Silica	2·16	2·3	7·3
Phosphoric acid.	8·40	8·5	7·3
Sulphuric acid	6·20	5·3	3·7
Carbonic acid	19·10	21·9	14·4
Chloride of sodium.	2·93	2·0	1·4
Chloride of potassium.	7·35		
Soda	. . .	1·3	6·9
Potass	26·50	25·8	13·0
Magnesia	4·76	5·9	5·5
Lime	22·30	27·0	40·0
	99·70	100·0	99·5

Beans.

Standard crop grown in a part of the Botanic Garden distinct from the portion set apart for the experiments.

100 grains yielded 6·45 of ash, 100 grains of which consisted of—

Sand and charcoal	12·00
Peroxide of iron	2·33
	14·33
Silica	2·44
Phosphoric acid	7·77
Sulphuric acid	2·95
Carbonic acid	17·38
Chloride of sodium	2·58
Chloride of potassium	0·91
Soda	0·00
Potass	30·37
Magnesia	2·69
Lime	17·17
	98·57
	14·33
Real ingredients	84·26

Beans.

Shifting crop.

100 parts of crop yielded 5·7 of ashes, 100 parts of which consisted of—

Sand and charcoal	8·24
Peroxide of iron	3·77
	12·01
Soluble silica	3·97
Phosphoric acid	3·83
Sulphuric acid	2·49
Carbonic acid	18·45
Chloride of sodium	1·23
Soda	0·22
Potass	20·56
Magnesia	3·79
Lime	33·87
	88·41
	12·01
Real ingredients	76·40

Beans.

Permanent crop.

100 parts of crop yielded 4·4 of ash, 100 parts of which consisted of—

Sand and charcoal, extraneous	6·13
Peroxide of iron, extraneous	4·24
	10·37
Soluble silica	4·05
Phosphoric acid	3·29
Sulphuric acid	1·96
Carbonic acid	19·87
Chloride of sodium	1·00
Soda	7·00
Potass	12·77
Magnesia	3·63
Lime	35·76
	99·70
	10·37
Real ingredients	89·33

The following Table will show the composition of the three crops of beans, after deducting the extraneous matters present in the ash.

Ingredients.	Standard.	Shifting.	Permanent.
Soluble silica	2·90	4·48	4·50
Phosphoric acid. . . .	9·25	4·32	3·68
Sulphuric acid	3·50	2·80	2·19
Carbonic acid	20·70	20·85	22·20
Chloride of sodium. . .	3·17	1·38	1·12
Chloride of potassium . .	1·08	0·00	0·00
Soda	0·00	0·24	7·80
Potass	36·10	23·20	14·20
Magnesia	3·20	4·28	4·06
Lime	20·30	38·20	40·00
	100·00	99·75	99·75

Having now, with reference to the six plants above-mentioned, stated, not only the amount of every year's crop, but also the composition of the last of each which had been obtained, we seem to be in a position to calculate the amount of the several inorganic ingredients contained in them, which will have been abstracted from the ground during the time the experiments were carried on.

This indeed is a question of little interest, so far as regards the acids and bases that are predominant ingredients in the soil, but in the case of the alkalies, the magnesia, and the phosphates, which exist there in more limited quantity, its determination may afford us a clew towards the main object of our inquiry, namely, the cause of the falling off of a crop after frequent repetition.

In the case of the barley, it will be seen, that the produce of the same plot of ground amounted in the course of ten years to 289·65 lbs., including straw as well as grain, and that, taking the last year's crop as the criterion, this quantity would have yielded 25·2 lbs. of ash.

For as 100—8·7—289·65—25·2.

Now 25·2 lbs. of ash would contain nearly as follows, according to the analyses given above :—

Sand and charcoal, Peroxide of iron } extraneous . . .	6·25
Silica	6·30
Phosphoric acid	1·84
Sulphuric acid	0·53
Carbonic acid	0·47
Chloride of sodium	1·18
Potass	4·36
Magnesia	1·15
Lime.	3·52
	25·60

On the other hand, the shifting crops of barley, which in ten years amounted to 421 lbs., and which, taking as our criterion the amount of ash yielded the last year (1844), had drawn from the land 26·31 lbs. of inorganic matter (the sand, charcoal, and peroxide of iron, drawn from other sources having been deducted), would have abstracted from the soil in ten years, as follows:—

Sand and charcoal, Peroxide of iron } extraneous . .	6·175
Silica	9·300
Phosphoric acid	2·370
Sulphuric acid	0·600
Carbonic acid	0·366
Chloride of sodium	0·364
Potass	4·220
Magnesia	0·915
Lime	1·960
	26·270

Proceeding now to the second case, that of the potatoes, we find the amount of the produce, in the case of the permanent crop, in nine years to have been 620·8 lbs., yielding 7·37 of ashes, of which, however, about 1·37 were extraneous. Consequently during that period the inorganic constituents, abstracted from the soil, and contained in 6 lbs. of real ash, would be as follows:—

Silica	0·11
Phosphoric acid	0·80
Sulphuric acid	0·27
Carbonic acid	0·80
Chloride of sodium . . .	0·20
Chloride of potassium . .	0·23
Potass	2·80
Magnesia	0·52
Lime	0·27
	6·00

On the other hand, we find, in the case of the shifting crop, the average of nine years' produce to be 89·1, or nearly 802 lbs. for the whole period, yielding of ashes about . . . . . 7·3 lbs.
from which must be deducted, as extraneous matter, about . . . . . 0·7

Leaving for real ash . . . . 6·6

and containing the following constituents:—

Silica	0·472
Phosphoric acid	1·070
Sulphuric acid	0·157
Carbonic acid	0·785
Chloride of sodium	0·130
Potass	3·300
Soda	0·055
Magnesia	0·452
Lime	0·178
	6·599

In the case of the next crop, the turnips, I have not sufficient data to determine with exactness the amount of inorganic ingredients extracted from the soil, having omitted to weigh the bulbs, from which the ash, in the case both of the permanent and shifting crops, was derived.

I find, however, that 1000 parts of a good sample from the neighbourhood of Oxford yielded 3·15 of ash, of which about 0·55 was extraneous, so that 2·6 grains will represent the amount of inorganic constituents really present.

The bulbs obtained from the permanent crop in ten years amounted to about 1008 lbs.; so that the inorganic constituents extracted from the soil in this instance may be reckoned at about 2·62 lbs.

Now 2·62 lbs. of inorganic matter would, according to the previous data, consist of the following ingredients, viz.

	lbs.
Silica	0·075
Phosphoric acid	0·360
Sulphuric acid	0·310
Carbonic acid	0·273
Chloride of sodium	0·050
Potass	1·110
Soda	0·000
Magnesia	0·110
Lime	0·332
	2·620

The shifting crop of turnips in the same period yielded of bulbs 1765 lbs., which, according to the same calculation, would have produced 4·58 of real ash.

For as 1008—262—1765—4·58.

Now 4·58 lbs. of ash would contain the following proportions of inorganic constituents, viz.

Silica	0·165
Phosphoric acid	0·540
Sulphuric acid	0·470
Carbonic acid	0·430
Chloride of potassium	0·270
Potass	1·930
Magnesia	0·255
Lime	0·520
	4·580

The next crop I shall consider is hemp, of which the permanent crop, according to the statements given in the first part of this paper, would have amounted in nine years to 271·25 lbs., or 30·13 lbs. per annum, yielding of ash 16·27 lbs.

For as 100—6·0—271·25—16·27.

Now 16·27 lbs. of ash would contain

of extraneous matter about 2·27

leaving 14·00 of inorganic principles belonging to the plant, which would consist of—

Silica	4·30
Phosphoric acid	0·70
Sulphuric acid	0·17
Carbonic acid	3·07
Chloride of sodium	0·06
Soda	0·08
Potass	1·10
Magnesia	1·40
Lime	6·12
	14·00

Now the average of the shifting crops for seven crops was 40 lbs., and as the ash obtained was about 7 per cent. its whole amount would have been 2·8 lbs.

	lbs.
Or in seven years	(2·8×7)=19·6
Of which the extraneous matter would be about	2·3
Leaving of inorganic principles extracted from the earth in seven years	17·3
Or, if in seven years—17·3—nine years	22·17

Now 17·3 lbs. of inorganic principles consist of

Silica	1·70	2·19
Phosphoric acid	1·18	1·52
Sulphuric acid	0·14	0·18
Carbonic acid	3·96	5·10
Chloride of sodium	0·12	0·15
Soda	0·03	0·04
Potass	1·47	1·90
Magnesia	1·00	1·29
Lime	7·60	9·80
	17·20	22·17

I next proceed to the flax, which in ten years produced, as we have seen, an amount of crop equal to 126 lbs., yielding of ashes 8·4 lbs.

of which 1·26 was extraneous.

Leaving of real ash 7·14, which would consist of—

Silica	0·520
Phosphoric acid	0·520
Sulphuric acid	0·264
Carbonic acid	1·020
Chloride of sodium	0·099
Soda	0·490
Potass	0·925
Magnesia	0·390
Lime	2·850
	7·078

Now the average of ten crops of flax, cultivated in different plots of the same garden, was 22·7 lbs., yielding 1·816 of ashes, = in ten years 18·16 lbs., of which 3·36 lbs. were extraneous, leaving 14·8 lbs. of real ash, which would consist of the following ingredients:—

Silica	0·34
Phosphoric acid	1·25
Sulphuric acid	0·78
Carbonic acid	3·22
Chloride of sodium	0·30
Soda	0·20
Potass	3·80
Magnesia	0·87
Lime	4·00
	14·76

The last of the crops made the subject of examination was the beans, where the aggregate of ten years' produce, in the case of the permanent crop, was 247 lbs., which would have yielded 10·8 lbs. of ashes.

Of this, however, about 1·1 would consist of extraneous matter.

Leaving 9·7 of real ash,

consisting of the following ingredients:—

Silica	0·44
Phosphoric acid	0·36
Sulphuric acid	0·22
Carbonic acid	2·13
Chloride of sodium	0·12
Soda	0·76
Potass	1·37
Magnesia	0·40
Lime	3·90
	9·70

In the case of the shifting crop of beans the produce of ten years gives an aggregate of 336 lbs., yielding of ash 19·15 lbs.,

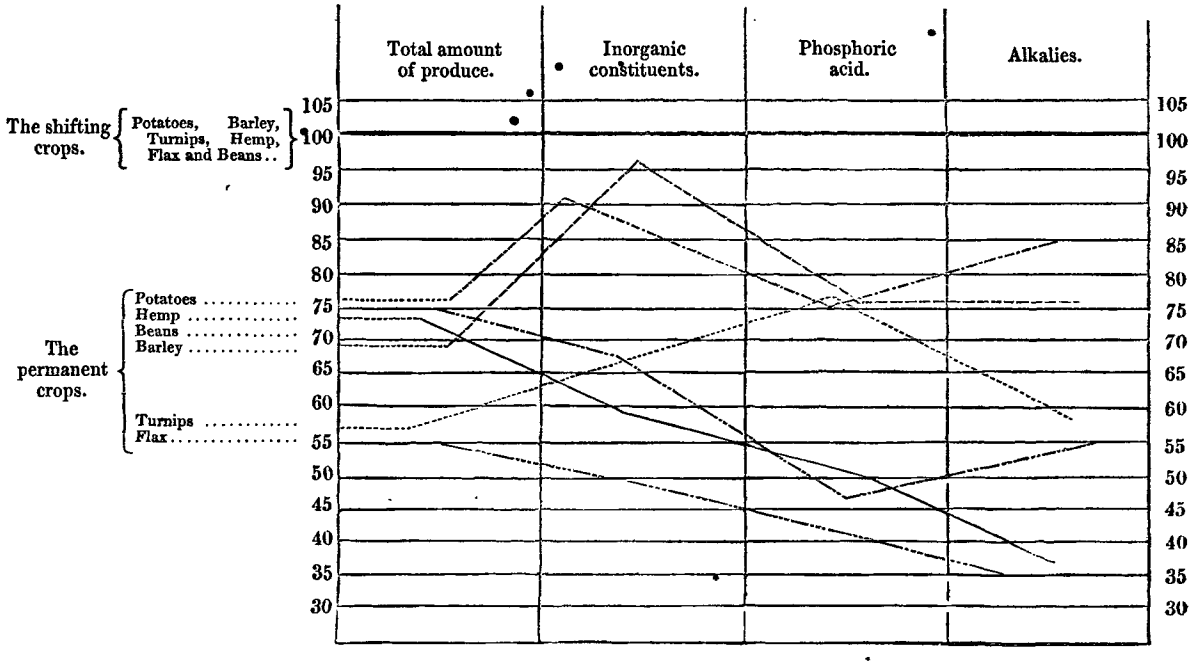
of which, however, about 2·30 was extraneous,

leaving 16·85 of real ash,

which would contain, of—

Silica	0·757
Phosphoric acid	0·723
Sulphuric acid	0·470
Carbonic acid	3·500
Chloride of sodium	0·235
Soda	0·044
Potass	3·900
Magnesia	0·725
Lime	6·420
	16·774

Diagram showing the relation between the Permanent and the Shifting Crops.



Before we proceed to inquire, whether the difference in the average amount of produce obtained under these two modes of cultivation arose from a deficiency of the organic, or of the inorganic materials present in the soil, it may be worth while to present a tabular view of the numerical relation subsisting between the permanent and shifting crops in each instance, with respect to the entire crop, to the entire amount of inorganic matter, and to the proportions of phosphoric acid and of alkalies present in each.

	Total amount of produce.		Inorganic constituents.		Phosphoric acid.		Alkalies.	
	Permanent.	Shifting.	Permanent.	Shifting.	Permanent.	Shifting.	Permanent.	Shifting.
Barley....	69·0	100·0	96·5	100·0	77·0	100·0	77·0	100·0
Potatoes..	77·5	100·0	91·0	100·0	75·0	100·0	85·0	100·0
Turnips ..	57·0	100·0	....	....	77·0	100·0	58·0	100·0
Hemp....	75·0	100·0	63·5	100·0	46·8	100·0	55·0	100·0
Flax.....	55·5	100·0	48·0	100·0	41·6	100·0	35·0	100·0
Beans....	73·0	100·0	58·0	100·0	50·0	100·0	36·0	100·0

The results in the two first instances would seem to lead to opposite conclusions from those suggested by the three latter, inasmuch as, whilst in the barley and the potatoes, the difference between the amount of inorganic constituents in the two cases was much less than that between the permanent and shifting crop collectively taken; in the hemp, flax and beans the contrary remark applies.

If we take the phosphoric acid, we find also that in the barley, and turnips, it stands in a higher ratio to the other constituents in the permanent, than in the shifting crop, whilst in the hemp, flax and beans, it stands in a much lower one.

A similar remark applies to the alkalies, so that no general conclusion, as it might seem, is deducible from these premises.

It appears to me, however, that the existence of a larger relative amount of phosphoric acid in the permanent than in the shifting crops of barley and of turnips, affords a stronger presumption in favour of a certain dependence of the produce on the organic matter, than the opposite result arrived at in the three other cases does of the reverse.

If the falling-off of the crop in these instances had arisen from a deficiency of certain of its inorganic principles, such for instance as the phosphates or the alkalies, at least a corresponding reduction in these latter might have been expected to have been found in the ashes of the one which proved deficient in quantity; whilst on the other hand, if the deficiency of organic matter be supposed to have checked the development of particular parts, as, for example, of the seeds, it might thereby affect the character of the ashes obtained, and thus a smaller amount be abstracted, without any actual failure, in the supply afforded by the soil to the plants that grew in it, taking place with regard to them.

I am led to this opinion, by the result of an examination, which I requested Mr. Way to institute, into the nature of the inorganic constituents present in ordinary gluten, and in starch.

The first, obtained from wheat, yielded about three parts of inorganic matter in the 1000 parts, which latter contained as much as 33 per cent. of phosphoric acid combined with lime, and a trace of magnesia, but no carbonate of lime*.

* I found also that the bran contained a larger proportion of silica than the albumen of the grain itself, and we know that the proportion of these several parts, one to the other, varies considerably in different samples of flour.

The latter, obtained from potatoes, yielded about 3·43 parts of inorganic matter in the 1000, of which only 4·77 per cent. was phosphoric acid, whilst 82·48 was carbonate of lime.

Thus, in the case of wheat, any condition of things which should check the formation of gluten, would diminish the quantity of phosphoric acid present in the ashes of this plant, even though the soil might contain an abundant supply of that ingredient; and as the formation of gluten is promoted by the presence of manures abounding in ammoniacal salts*, so it may easily happen, that this principle should be deficient where such manures are too sparingly administered.

In a similar way, a variation in the constituents of barley and other crops may be supposed to arise, not only from a larger or smaller supply of inorganic principles in the soil, in the manner that LIEBIG has so lucidly explained to us, but likewise from a more plentiful exhibition of those products of the decomposition of organic bodies, which favour the development of particular organs, or of certain of the proximate principles which the latter contain.

Which, however, of these two suppositions applies to the cases now under consideration, will be better seen, when we have considered the composition of the soil in which they grew, as determined by analysis.

PART III.

On the chemical composition of the soil in which the crops were grown, and on the proportion of its ingredients that were available for the purposes of vegetation.

The chief difficulty, which occurs with respect to the analysis of a soil, relates to the determination of those ingredients which, like the phosphates and the alkalies, exist in minute proportions, and which accordingly appear to have been overlooked by DAVY, and others, who first applied themselves to the subject of agricultural chemistry.

It will not be necessary therefore to take up the time of this Society, by giving a detailed account of the method pursued by Mr. WAY in his examination of the soils of which I wished to learn the composition; it may be sufficient to state, that after separating the several portions, one from the other, by the mechanical method pointed out by Mr. RHAM, and determining the relation which the coarser bore to the finer, the latter, which alone were supposed capable of imparting any nourishment to plants, at least within a limited period, was submitted to the usual course of examination pursued by chemists.

To ascertain the phosphates however, a distinct and a much larger portion of the soil was operated upon, not less than 2000 grains being taken for the purpose, and this was digested for five hours in water acidulated with muriatic acid, the flask employed for the purpose being fitted up with a funnel attached to its neck, in the manner re-

* See HERMBSTADT's experiments quoted in the third of my Lectures on Agriculture, and Sir H. DAVY's Lectures on Agricultural Chemistry.

commended by Dr. URE, for the purpose of condensing the acid which might be disengaged in vapour, and restoring it to the body of the vessel*.

The liquor, after being filtered, was evaporated to dryness, so as to dispel the greater part of the acid.

The residuum was then treated with water, and an excess of ammonia was added, by which the iron, alumina, and phosphate of lime were thrown down.

The whole was then carried to dryness, and gently ignited, by which means the greater part of the iron and of the alumina is rendered insoluble in dilute acids, which take up the phosphate of lime.

The solution was then treated with ammonia so long as any precipitate was thrown down, and the latter digested with dilute alcohol mixed with sulphuric acid, by which any alumina and iron that had been precipitated were converted into soluble salts, whilst any lime in combination with phosphoric acid would remain as an insoluble sulphate, from the amount of which, when well-washed and dried, that of the phosphate present in the soil admits of being calculated.

After ammonia had thrown down the alumina, iron, and phosphate of lime, the alkalies existing in the ash would still remain in the solution.

The latter was therefore again evaporated to dryness, and the ammoniacal salts driven off.

The residue was then treated with water, boiled and filtered, after which a solution of carbonate of ammonia, to which a little pure ammonia had been added, was introduced into the liquor that came through. The remainder of the earths were thus thrown down, and nothing remained in solution except the alkalies. After the ammoniacal salts had been expelled by heat, the mixed chlorides of potassium and sodium were separated in the usual way by chloride of platinum.

Such then was the method pursued for determining the nature and proportions of those ingredients, which, if not available for the purposes of vegetation at the present time, may at least be regarded as likely to prove useful to them within no very distant period, as being separable, by dilute muriatic acid, from the mass of the earth.

The soil of that part of the garden, in which the experiments above detailed had been conducted, varied in depth from three to four feet, and rested upon a stiff clay, of which the subsoil in the valley of Oxford consists, wherever it is not overlaid by gravel.

It was chiefly made ground, brought in to elevate the level of the garden above that to which the contiguous river rises during the winter floods, and about a year antecedent to the commencement of the experiments it had been manured with stable dung.

I have already expressed my regret, that no analysis was made of it until the present year, at which time the experiments had been already brought to a close.

In a neighbouring part of the garden, which appeared to be similarly circumstanced to that which had been set apart for the experiments, except that it had been recently

* Journal of the Agricultural Society, vol. v. p. 617.

manured, the ingredients of the impalpable portion of the soil, which had passed through the meshes of a fine sieve, were found by Mr. Way to be as follows:—

Sand and clay	410·44
Organic matter	103·96
Silica	95·06
Lime	139·53
Peroxide of iron	98·33
Alumina	35·60
Carbonic acid	106·10
Sulphuric acid	1·82
Chlorine	a trace.
Magnesia	·46
Potass	2·58
Soda	1·18
Phosphate of lime	·73
	995·79
Loss	4·21
Coarse sand	890·00
Stones and pebbles	870·00
	2760·00

The soil of the garden in which the experiments had been conducted was also subjected to a similar examination, the plot which had grown a crop of barley for ten years without manure having been selected.

The following were the results:—

Sand and clay	407·00
Organic matter	75·00
Silica	109·20
Lime	144·17
Peroxide of iron	103·80
Magnesia	·85
Alumina	25·00
Sulphuric acid	1·65
Carbonic acid	125·69
Potass	2·91
Soda	·29
Phosphate of lime	·80
Chlorine	a trace.
Loss	3·64
	1000·00
Coarse sand	810·00
Stones and pebbles	660·00
	2470·00

In another of the beds which had reared a permanent crop, viz. that of potatoes, the proportion of the phosphates, alkalies, and magnesia did not appear to vary much, the analysis of 1000 grains of the finer portion sifted, affording the following results:—

Phosphate of lime . . .	0·86
Potass	1·57
Soda	0·27
Magnesia	0·82
The organic matter here was	53·00

A third of the beds in the same garden, which had borne a crop of turnips for ten years, exhibited rather a remarkable anomaly, as the phosphates exceeded in quantity considerably that present in the contiguous garden, the results being as follows:—

Potass, in 1000 parts . .	0·46
Soda	0·74
Phosphate of lime . . .	1·62
Organic matter . . .	110·80

It will be seen, that the permanent crop of turnips in ten years would have extracted from the soil only 0·36 of phosphoric acid, whilst the barley in the same time had extracted 1·84, and the potatoes 0·80; hence perhaps the difference in the quantity present in the soil.

One only of the plots of ground, which had grown a succession of different crops for ten years without manure, was examined*, and the proportion of the above ingredients found in it appeared to be as follows, viz.—

Potass in 100 grains . .	1·96
Soda	1·12
Phosphate of lime . . .	0·33
Organic matter	76·50

It will be seen from the table in the following page, that, taking as our standard the composition of the contiguous garden, of which the analysis is first reported, and in which the proportion of phosphoric acid would seem to be lower than it is in most of the plots of ground experimented upon, even after ten years' cropping without manure (judging from the few which were examined), a sufficient quantity of the above ingredient existed, to supply what would be necessary for nineteen crops of barley, of the same amount as the average of those obtained from the permanent bed, and of the same quality as that produced in 1844.

* The crops were, barley in 1844, hemp in 1843, buckwheat in 1842, tobacco in 1841, parsley in 1840 and 1839, mint in 1838 and 1837, parsley in 1836, and beans in 1835.

In the following Table the results are all reduced to one standard.

1000 grains of the soil.

Ingredients.	Contiguous garden.	Permanent bed of barley.	Permanent bed of potatoes.	Permanent bed of turnips.	Bed which had borne a succession of ten crops without manure.
Stones and pebbles . .	315·000	267·000			
Coarse sand	322·000	328·000			
Fine sand and clay . .	149·000	165·000			
Organic matter	37·600	30·400	21·400	44·8*	31·000
Silica, soluble	34·400	44·000			
Lime	50·500	58·000			
Peroxide of iron . . .	35·500	41·800			
Alumina, soluble . . .	12·800	10·100			
Carbonic acid	38·400	50·500			
Sulphuric acid	0·660	0·665			
Chlorine	a trace.	a trace.			
Magnesia.	0·167	0·344	0·332		
Potass.	0·930	1·180	0·640	0·186	0·790
Soda	0·428	0·117	0·109	0·300	0·470
Phosphate of lime . . .	0·265	0·322	0·346	0·656	0·133
	997·650	997·428			

For one cubic foot of the soil of the garden was found to weigh eighty-two lbs., from which it follows, that an area of 100 square feet to the depth of three feet (which is less than the average depth of the soil in the garden), would contain 24,600 lbs. of soil, which at 1·0 gr. to 1 lb. of soil would give an amount of phosphoric acid equal to 3·5 lbs.

Now it has been calculated (page 230) that the quantity of phosphoric acid extracted from the soil in ten years did not exceed 1·84 lb., so that the permanent bed of barley, which contains at present 0·8 of phosphate of lime, or 0·4 of phosphoric acid, would not have possessed before the cropping more than 1·26 of phosphate, or 0·63 of phosphoric acid, in the 1000 grains.

With respect to the alkalies, we shall find by the same mode of calculation that the medical garden contains in 100 square feet—

	lbs.
Potass	6·9
Soda	2·9

And as each permanent crop of barley in the average extracted no more than of—

Potass	0·436
Soda	0·064,

(deduced from 1·18 of chloride of sodium) in 100 parts, there would be a supply of potass equal to fifteen crops of barley, and of soda equal to forty-five crops.

* Thus we perceive that a series of ten successive crops of turnips had added more organic matter to the soil than it had abstracted. See BOUSSINGAULT's late work, chap. vii. on the Rotation of Crops.

Here also we have reason to believe, that the soil of the experimental garden was richer than that upon which our calculations are founded, so that the falling-off of the crop cannot be attributed to any actual deficiency either of alkali or of phosphoric acid in the soil.

The amount of magnesia in the soil was also very small, not exceeding 3·8 lbs. to the 100 square feet.

This however would have been sufficient for thirty-four crops of barley, according to the estimate given (in p. 230) of the quantity taken up by the crop in ten years.

When, however, we proceed to inquire into the quantity of these ingredients, which are at the particular moment in a condition to be taken up by the spongioles of the roots, we find the case very different.

I have already pointed out, that, with a view of imitating nature as nearly as possible, water impregnated with carbonic acid is a preferable solvent to muriatic acid, since it may be presumed, that what is not extracted from the soil by a sufficiently large amount of the former, is not in a condition to be readily assimilated by the plants that grow in it.

I therefore took sifted portions, each weighing 5 lbs., of the soil, from the part of the garden contiguous to the scene of my experiments, as well as from several of the plots which had grown either the same or different crops during ten years without the addition of manure, and having introduced them into earthen pots, with a hole at the bottom covered over with a piece of wire gauze fine enough to prevent the earth from falling through, I added to each a known quantity of distilled water which had been saturated with carbonic acid gas.

After a certain amount of the water had passed through, generally two quarts were taken and evaporated to dryness, after which the residuum was treated, in the first place with water, which took up the alkaline salts together with a little calcareous matter, and afterwards with muriatic acid, which dissolved the rest of the lime, whether in combination with carbonic or with phosphoric acid.

Having got rid of the earthy matter from the aqueous solution by means of oxalate of ammonia, the alkalies remaining were converted into sulphates, heated and weighed, after which the nature of the alkali, combined with the sulphuric acid, was determined by the usual method.

The acid solution was then treated with ammonia, and the precipitate, when well-washed and dried, was set down as phosphate of lime, iron being rarely present, and never except in minute proportions.

The following results were obtained, by operating in this manner on the soils enumerated below, and, granting that objections may be raised against the precision of the method adopted, they at least suffice to show, that the ten years' cropping had reduced very materially the amount of matter immediately available for the purposes of vegetation, however little it appear to have trenched upon the latent resources of the soil.

Table of the quantity of alkaline sulphates and earthy phosphates extracted by means of water impregnated with carbonic acid from the soils enumerated below.

Soil examined and treated with water.	Quantity of water added.	Quantity of alkaline sulphate obtained.	Nature of the alkali.	Quantity of alkaline sulphate per quart of water.	Quantity of alkaline sulphate in 1 lb. of soil.	Quantity of alkaline sulphate in 100 square feet (24,600 lbs.) of the soil.	Quantity of earthy phosphate taken up.	Quantity of earthy phosphate per quart of water.	Quantity of earthy sulphate per lb. of soil.	Quantity of earthy phosphate in 100 square feet of soil (24,600 lbs.).
	qts.	gr.		gr.	gr.	gr.	gr.	gr.	gr.	gr.
From the contiguous garden, first time	2	5·2	Potass.	2·6	.....	.....	0·7	0·35		
From the contiguous garden, second time ..	2	7·8	Potass.	3·9	.....	.....	0·7	0·35		
From the contiguous garden, third time	1	3·4	Potass.	1·7	.....	.....	0·05	0·05	0·29	7134
From the contiguous garden, fourth time ..	1	2·6	Potass.	1·3	3·4	83640				
From the permanent bed of Barley.....	2	0·6	Soda.	0·30	0·12	2950	0·30	0·15	0·06	1470
From the permanent bed of Potatoes	2	0·7	Soda.	0·35	0·07	1700	0·25	0·125	0·05	1200
From the permanent bed of Hemp	2	0·6	Soda.	0·30	0·12	2950	Scarcely appreciable.			1470
From the permanent bed of Flax.....	2	0·5	Soda.	0·25	0·10	2450	Scarcely appreciable.			2940
From the permanent bed of Turnips	2	0·6	Soda chiefly.	0·30	0·12	2950	0·30	0·15	0·06	3180
From the permanent bed of Beans	2	0·5	Soda.	0·25	0·10	2450	0·60	0·30	0·12	4900
From the shifting bed of Barley	2	0·7	Soda chiefly.	0·37	0·07	1700	0·065	0·0325	0·013	3420
From the shifting bed of Potatoes	2	1·0	Soda chiefly.	0·50	0·20	4900	0·100	0·050	0·020	4900
From the shifting bed of Hemp	2	1·0	Soda chiefly.	0·50	0·20	4900	Scarcely appreciable.			
From the shifting bed of Flax.....	2	0·3	Soda chiefly.	0·15	0·06	1470	0·7	0·35	0·14	3420
From the shifting bed of Turnips	2	3·6	Potass.	1·8	0·72	17700	0·9	0·45	0·18	4410
From the shifting bed of Beans	2	1·0	Soda.	0·50	0·20	4900	0·30	0·15	0·06	1470

Thus it is seen, that whilst the entire quantity of phosphate present in 100 square feet of the garden soil amounted to 43911 grains, or exceeded 6 lbs., and that of alkali (including both potass and soda) to 233,700 grains, exceeding 33 lbs., all that could be extracted from the same quantity of soil by water was, 7134 grains of phosphate, and an amount of alkali sufficient to produce 83,640 grains of sulphate.

It appears, moreover, that, in the soils which had been drawn upon for ten years, either by the same crop or by a succession of different ones, without the application of manure, whilst the actual amount of phosphate and of alkali was fully as great as in the other parts of the garden, the quantities extracted by water were many times less; and although it need not be supposed, that what had been withdrawn by two quarts of water constituted the whole amount of these substances which was available at the time for the purposes of vegetation, yet it seems probable, that the facility with which the above ingredients were supplied to the plants, would bear some relation to the quantities taken up by the same amount of water from the different soils.

Since, therefore, the amount of phosphates and of alkalies extracted by two quarts of water in these cases falls considerably short of the quantities of those ingredients required for an average crop of barley, such as that produced for ten years in succession in the same soil, it may be fairly concluded, that the deficiency in the produce arose in part from a less ready supply of these constituents being provided, than would have been the case in soil newly broken up, or recently manured, where, although the absolute amount of nutritious principles may not be very different, the proportion of them in a state directly applicable to the uses of the plant will be much greater.

This hypothesis however seems to me only to afford a partial explanation of the problem before us; for repeated instances occur in this paper, of two soils presenting no apparent difference in the condition of their ingredients as to solubility, and in other respects alike, which nevertheless have varied very materially in the amount and quality of their produce, according as the crop has been a permanent or a shifting one; so that in these instances, the crop had extracted different quantities of phosphates and of alkalies from two soils, both of which were capable of supplying them with these principles, with equal readiness, and in equal abundance.

This circumstance might seem to favour the idea, that the quantity and condition of the organic matter present in the soil may exercise some control over the development of the crop.

Upon the whole, then, it must, I think, be admitted, on the one hand, that the quantity of inorganic matter brought into a soluble condition would, other things remaining the same, be more considerable in proportion to the activity with which the processes of vegetation are carried on, inasmuch as those operations which result from the vitality of the plant, would facilitate that introduction of air and water into the body of the soil, by which a fresh portion of the above ingredients might be brought into a more soluble condition—owing to the separation of the clods of earth,

caused by the fibres of the roots insinuating themselves amongst them—owing to water impregnated with carbonic acid, excreted by the extremities of the roots, which may exert its solvent power upon the principles contained in the soil—and owing to an imbibition, by the plants themselves, of the water surrounding them, which would cause a general movement and circulation in the fluid contained in all the portions of the soil contiguous.

On the other hand, it would seem, that a due supply of these necessary ingredients, already prepared and available for their purposes, would itself be likely to favour the development of the parts of the vegetable, and thus to cause a larger portion of such substances to be extracted from the earth, by the more vigorous action excited within the secreting organs themselves.

These effects are so connected together, that it is difficult to pronounce which of them deserves to rank as the first link in the series.

The only inferences, therefore, I could venture at present to deduce from the facts which I have laid before the Society, are as follows:—

1st. That it is quite consistent with the general tenor of the preceding facts and observations, to maintain with BOUSSINGAULT, that the falling-off of a crop is dependent upon a deficiency of organic matter proper to promote the nutrition of the plants, as well as upon a failure of its inorganic principles; not indeed that the organic matter enters, as such, into the constitution of the vegetable, but that by its decomposition it furnishes it with a more abundant supply of carbonic acid and ammonia, which supply accelerates the development of its parts, and thus at once enables it to extract more inorganic matter from the soil, and enables the soil to supply it more copiously with the principles it requires.

Hence, perhaps, in part, the advantage of intercalating the Leguminosæ and other fallow crops, which generate a larger amount of organic matter than the Cerealia, and which thus serve to enrich the soil by what they leave behind them.

2ndly. That it by no means follows, because a soil is benefited by manuring, even though that manure may, as in the case of bones, guano, &c., derive its efficacy from the phosphates it supplies, that the soil is therefore destitute of the ingredient in question, since it may happen, that it possesses abundance of it in a dormant, though not in an immediately available condition.

In these cases, in which the agriculturist has been assured by the results of actual analysis, that there is no real dearth of the principles essential to his crops in the soil which he is cultivating, but where he has ascertained, either by the chemical mode pointed out, or by an experience of the good effects brought about by manures, that the principles in question are not in a state to become immediately applicable to the purposes of vegetation, three courses appear to be open to him:—

1st. To apply a sufficient quantity of the same materials in a state in which they can be absorbed by the plants without delay; 2ndly, to allow the ground to remain fallow, by which expedient time is allowed for a further decomposition of its mate-

rials, and for a renewed extrication of its useful ingredients, to take place; 3rdly, to produce by the various methods in daily use, such a stirring and pulverization of the ground, as may admit of a more thorough admission of air and moisture, and consequently accelerate the process of disintegration in a greater degree than would take place under natural circumstances.

Examples will occur to every one of the successful adoption of each of these three practices: of the first, in the ordinary process of manuring, and especially in the beneficial consequences resulting from the use of bones in the exhausted pastures of Cheshire and other similar localities; of the second, in the system so general in the early stages of agriculture, of allowing land to remain at rest for a certain period with a view of restoring to it its exhausted powers,—a method which would be absurd, if the alkalies, phosphates, and other of the more scanty ingredients were absolutely deficient, but which would be likely to prove efficient, if they were only locked up within the recesses of the soil, and required time to render them active; of the third, in the practice resorted to by JETHRO TULL, who boasted that he could realize an abundant crop year after year without manure, provided the ground were only stirred and broken up sufficiently,—a statement which seems confirmed, by some of the results of spade husbandry, and in a certain degree by those detailed in this paper, with respect to the permanent crops which are herein mentioned as having been made the subject of experiment.

The choice between the above three methods will of course be determined in each instance by a balance of economy, and although in general this latter consideration will incline the farmer to prefer the ordinary method of manuring, either to the sacrifice of a year's produce, as in the second method, or to the expenditure of labour required to put into practice the third, still there may be cases where it might better answer his purpose to resort to one or other of them, either as being more advantageous in itself, or more suitable to the circumstances of the case.

At any rate it may be important for him to be assured, that at the very time he is ransacking the most distant quarters of the globe for certain of the mineral ingredients required for his crops, he has lying beneath his feet in many instances an almost inexhaustible supply of the very same.

For there seems no reason to doubt, that the whole mass of rock, which constitutes the subsoil in the secondary and tertiary districts of this country, is as rich in phosphates and in alkalies, as the vegetable mould derived from its decomposition; and although the soil, in which the experiments in my garden were conducted, possessed a depth nearly three times as great as the average of those in which farm produce is generally raised, yet on the other hand, the amount of phosphates and of alkaline ingredients reported to be present in them, appears in many instances greater than that determined in the case before us.

Thus Dr. URE* gives an analysis of a soil in the parish of Hornchurch, Essex,

* Journal of the Royal Agricultural Society.

which contained four grains of phosphate of lime in 1000 grains, whereas ours scarcely exceeded one-fourth of a grain in the same quantity; and if the former be regarded as an exceptional case, I might refer to SPRENGEL, who states, that the per-centage of phosphoric acid in the soils he analysed varied from 0·024 to 0·367; and of the subsoil from about 0·007 to 0·2.

I detected many years ago phosphate of lime in several secondary limestones chiefly from the oolitic formation, and Mr. SCHWEITZER of Brighton has determined the proportion of that ingredient in the chalk near Brighton, to be not less than one grain in the 1000. We need not therefore resort to South America for bones, if means could be found for extracting this ingredient economically from the rocks of our own country.

3rdly. These facts place in rather a new light, although one, it is conceived, not less striking than before, the importance of taking care of the various excrementitious matters at our disposal, whether proceeding from animal or from vegetable sources.

Such substances indeed contain the products, which nature has, with so large a consumption of time, and by such a number of complicated operations, elaborated from the raw material contained in the soil, and has at length brought into the condition, in which they are most soluble, and therefore best fitted to be assimilated by the organs of plants.

To waste them, is therefore to undo, what has been expressly prepared for our use by a beautiful system of contrivances, and to place ourselves under the necessity of performing, by an expenditure of our own labour and capital, those very processes, which nature had already accomplished for us, without cost, by the aid of those animate or inanimate agents which she has at her disposal.

4thly. The analyses above reported may suggest caution as to the inferences which some might be disposed to deduce from certain researches lately announced, with respect to the power which a plant possesses of substituting one alkali, or one earth, for another, in the processes of vegetation.

This substitution indeed, however brought about, is a fact which hardly admits of being questioned, supported as it is by the testimony of men so eminent as SAUSSURE and as LIEBIG, and indeed many of the analyses detailed in this paper might be appealed to in corroboration of its truth.

Thus we find, that whilst the amount of bases agreed pretty nearly in the three crops of the same plant which had been analysed, the proportions between them often varied considerably. This is particularly seen in the case of the lime and magnesia, the deficiency in one of these earths being often made up by an excess in the other.

In like manner a deficiency of potass is found to be compensated by an increased amount of soda, and the same remark seems to apply to the acids.

Still we have not as yet sufficient data for determining to what extent this exchange of the usual ingredient for another can take place; whether indeed the same

organ, or the same proximate principle belonging to the plant, may admit at all of this change in its constitution taking place; or if it can, in what degree the presence of this new principle may affect the healthy development of the vegetable.

By turning to the Table which states the relative quantities of alkaline ingredients extracted from the different soils by water impregnated with carbonic acid, it will be seen, that in most of these the amount of soda predominated over that of potass, and yet the latter alkali was principally found in their ashes, an indication at least of some superior adaptation of potass to soda for the organization of plants*.

Again, it is remarkable, that whilst in several of the soils soda appeared to exist in the form of a carbonate (since the quantity of chlorine was so small that only a minute trace of it was discoverable in them), in many of the ashes of the plants, only as much soda was detected as would contain sodium equivalent to the chlorine present.

Hence it would seem to follow, that common salt, when it acts beneficially upon land, does not assist the crop by virtue of the alkali it imparts to it, but in some other way, and that it is still questionable, at least in the case of terrestrial species, whether plants have the power of decomposing chloride of sodium, and of separating its chlorine.

Lastly, the analyses contained in this paper may be of use at the present moment, by contributing to show, how much still remains to be done, before we can flatter ourselves at having attained any sure knowledge of the normal constitution of plants, and of the range of variation of which under natural circumstances it is susceptible. At a time when certain enlightened members of the Royal Agricultural Society have prevailed upon that great Body, to devote a portion of their funds to the prosecution of the chemical analysis of the ashes of vegetables, whatever tends to render more palpable the importance of such an investigation, may be of service, in aiding their meritorious efforts, to give a more scientific direction to the inquiries which such associations are intended to promote, and in vindicating the utility of the course which they have in this instance adopted.

Now the facts and observations detailed in the present paper contribute in two respects towards this object, viz. by showing that the composition of the most commonly cultivated plants is still open to much uncertainty; and 2ndly, by pointing out in what way an exact knowledge of their inorganic ingredients might aid us towards the solution of many important practical questions.

I hope, it will not be attributed to any blindness on my part to the deficiencies and imperfections which exist in this paper, if I remark, that an investigation of a similar kind to the one herein detailed, if carried out on a more adequate scale, undertaken on ground more carefully selected, conducted with a more vigilant attention to all the minute circumstances which might influence the result, and accompanied by a

* This is also shown very strikingly in a paper on the analysis of Fuci, read to the British Association at Cambridge, by Mr. Schweitzer, in June 1845.

regular series of analyses, both of the soil and of the crops, during the whole period of their continuance, would be of essential service in clearing up many points which yet remain open to investigation in agricultural science.

My Memoir may serve also as a kind of illustration of that method of Scientific Book-keeping, which I proposed some time ago, at once as an useful exercise for the agricultural student, and as a means of introducing greater precision in the conduct of our experiments on this subject, and which I am therefore happy in having this opportunity of rendering more generally known.

VIII. *An Account of the Artificial Formation of a Vegeto-Alkali.*

By GEORGE FOWNES, *Ph. D., F.R.S., Chemical Lecturer in the Middlesex Hospital Medical School. Communicated by* THOMAS GRAHAM, *Esq., F.R.S., &c.*

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A FEW months ago Mr. MORSON very kindly put into my hands, for examination, a quantity of dark-coloured, viscid oil, amounting to six or seven ounces, which was said to have been produced by the action of sulphuric acid upon bran. The tarry appearance of the oil was evidently the result of oxidation, for the bottle in which it had been preserved during a period of five years was very imperfectly closed, while a second and smaller portion, which had been kept in a stoppered bottle the same length of time, although dark in colour, was perfectly thin and fluid.

A portion of the oil was introduced into a retort, together with a quantity of water, and the whole submitted to distillation; water, accompanied by a heavy, pale-yellow volatile oil, came over. At the close of the process the retort was found to contain a solid, pitchy residue, insoluble in water, but dissolved in great measure by caustic potash, and again precipitable by the addition of an acid.

The distilled oil, separated by a funnel from the water under which it rested, after having been left a few days in contact with fused chloride of calcium, was distilled alone in a small retort fitted with a thermometer, the bulb of which dipped into the liquid. A little water came over at first with the oil, but this quickly ceased to appear, and then the temperature of ebullition remained quite constant to the close of the distillation, which was conducted nearly to dryness. It was inferred from this experiment that the oil was a single substance, and not a mixture of two or more different bodies.

The water which came over with the oil in the first distillation contained a very considerable quantity of that substance in a state of solution; it was strongly acid besides, from the presence of formic acid.

The purified oil was next submitted to analysis in the usual manner, by combustion with oxide of copper. The following were the results:—

	(1.)	(2.)	(3.)
Oil employed	5·73 grs.	7·79 grs.	5·547 grs.
Carbonic acid produced	13·18 grs.	17·74 grs.	12·64 grs.
Water produced . . .	2·27 grs.	2·96 grs.	2·12 grs.

Hence the composition in 100 parts*, nitrogen being altogether absent,—

* The equivalent of carbon is taken throughout =6.

	(1.)	(2.)	(3.)
Carbon	62·73 grs.	62·11 grs.	62·14 grs.
Hydrogen	4·40 grs.	4·22 grs.	4·24 grs.
Oxygen	32·87 grs.	33·67 grs.	33·62 grs.
	100·00	100·00	100·00

The formula $C_{15} H_6 O_6$, calculated to 100 parts, gives numbers almost coincident with the above, viz.—

Carbon	62·50
Hydrogen . . .	4·17
Oxygen	33·33
	100·00

The following is a summary of the chief properties of the oil :—

When free from water and freshly rectified, it is nearly colourless, but after a few hours acquires a brownish tint, which eventually deepens almost to blackness; when in contact with water, or when not purposely rendered anhydrous, it seems less subject to change, and merely assumes a yellow colour. Its odour resembles that of a mixture of bitter almond oil, and oil of cassia, but has less fragrancy. The specific gravity of this substance at 60° is 1·168; it boils at 323° Fahr., and distils at that temperature without alteration. Cold water dissolves the oil in question to a very large extent; by distillation, and fractioning the products, it can be again separated. In alcohol it dissolves with perfect facility. Concentrated sulphuric acid dissolves it in the cold with magnificent purple colour; the solution is decomposed by water, with separation of the oil. If heated with the acid, it is charred and destroyed with evolution of sulphurous acid. Strong hydrochloric acid behaves in a very similar manner. Nitric acid, by the aid of a little heat, attacks the oil with prodigious violence, evolving copious red fumes, and generating oxalic acid, which appears to be the only product.

Solution of caustic potash, in the cold, slowly dissolves the oil, forming a deep brown liquid, from which acids precipitate resinous matter; by the aid of heat, the same change ensues very rapidly. Metallic potassium was found to be slowly acted upon by the cold oil, but on slightly elevating the temperature, an explosion took place, attended by a voluminous flame and a large deposit of soot, my hand being at the same time severely burned by a portion of the projected potassium.

The most remarkable and characteristic reaction of this substance, however, is with ammonia. When placed in contact with five or six times its bulk of ordinary *liquor ammoniæ*, and left some hours, it is gradually, but in the end completely converted into a solid, yellowish-white, and somewhat crystalline mass, which is very bulky, and perfectly insoluble in cold water. It may be easily collected on a filter, drained from the ammoniacal mother-liquor, and dried *in vacuo* over a surface of oil of vitriol. This substance appears to be the only product of the action of ammonia on the volatile oil.

Portions of the new body, prepared in the manner above stated, and very carefully dried over sulphuric acid, gave the following results on analysis by combustion with oxide of copper. The proportion of nitrogen was determined by the excellent method of MM. WILL and VARRENTRAPP.

	(1)	(2.)	(3.)
Substance employed . . .	7·94 grs.	6·37 grs.	4·195 grs.
Carbonic acid produced .	19·34 grs.	15·57 grs.	10·24 grs.
Water produced	3·24 grs.	2·59 grs.	1·69 grs.

Hence, in 100 parts—

	(1.)	(2.)	(3.)
Carbon	66·55 grs.	66·66 grs.	66·57 grs.
Hydrogen	4·53 grs.	4·52 grs.	4·47 grs.

Determination of nitrogen*:—

	(1.)	(2.)	(3.)
Substance employed . . .	6·02 grs.	4·65 grs.	4·45 grs.
Platinum-salt obtained . .	10·10 grs.	7·62 grs.	7·17 grs.
Per-centage of nitrogen .	10·58	10·54	10·16

These results lead directly to the formula $C_{15}H_6NO_3$; when this is reckoned to 100 parts, it gives—

Carbon	67·13
Hydrogen . . .	4·47
Nitrogen . . .	10·48
Oxygen	17·92
	100·00

Hence it is clear that the solid substance is produced from the oil by the assimilation of the elements of one equivalent of ammonia, and the separation of those of three equivalents of water.

$$\left.\begin{array}{rr} \text{Oil} . . . . . & C_{15}H_6\ O_6 \\ +1 \text{ eq. ammonia} . & H_3N \end{array}\right\} = \left\{\begin{array}{ll} \text{solid} . . . . . & C_{15}H_6NO_3 \\ 3 \text{ eq. water} . . . & H_3\ O_3 \end{array}\right.$$

$$C_{15}H_9NO_6 \qquad\qquad C_{15}H_9NO_6$$

The new substance belongs in fact to the class of the *amides*, which it resembles in many important particulars, as will be more apparent in the sequel.

In the year 1841 Dr. STENHOUSE† published an interesting paper on a peculiar oily matter, first noticed by DOEBEREINER, which occasionally appears in very small quantity, in the preparation of artificial formic acid from sugar or starch, oxide of manganese, and dilute sulphuric acid, and to which he gave the name of *artificial oil of ants*. Dr. STENHOUSE, in the investigation above referred to, succeeded in pre-

* It may not be amiss perhaps to mention, that in the method adopted the organic substance is strongly heated in a hard glass tube with a mixture of hydrate of soda and quicklime. The whole of the nitrogen issues thence in the form of ammonia, and being condensed into hydrochloric acid, is afterwards converted into chloride of platinum and ammonium, from the weight of which that of the nitrogen is easily calculated.

† Philosophical Magazine for February 1841; also Annalen der Chemie und Pharmacie, xxxv. p. 301.

paring this substance at will, and in sufficient quantity to serve the purposes of experiment; he found it composed of carbon, hydrogen and oxygen, and, what appeared very extraordinary in such a body, the two latter elements were in the proportions to form water. The analytical results, reckoned to 100 parts, were as follows:—

	(1.)	(2.)	(3.)
Carbon	62·59	61·87	62·55
Hydrogen . . .	4·37	4·37	4·46
Oxygen	33·04	33·76	32·99
	100·00	100·00	100·00

These numbers agree very closely with those furnished by the assigned empirical formula $C_5 H_2 O_2$.

There could be but little doubt that the oil upon which I had been experimenting was identical with the substance above described; its properties agreed on the whole pretty well with those assigned by Dr. STENHOUSE to his interesting product, with the exception of its extraordinary behaviour with ammonia, which seems to have escaped notice, probably from time being required for the production of the *amide*. To complete the identification, therefore, a small portion of oil was prepared, the process described and recommended being closely followed. 2 lbs. of oatmeal, 2 lbs. of water, and 1 lb. of oil of vitriol, were well-mixed in a small copper still, and heated until the pasty mass became thin and fluid from the conversion of the starch into dextrine; the head was then applied and luted down, and distillation commenced. As soon as sulphurous acid began to appear, an additional lb. of water was introduced, and the distillation continued until that gas began to escape in large quantity. The liquid which came over was then rectified to one-half, the product neutralized with hydrate of lime to fix the sulphurous and formic acids, and again distilled, the first third only being collected. A small quantity of heavy yellow oil was thus procured, and an additional portion obtained by again subjecting the watery liquid to distillation. So far as could be seen, this oil corresponded in every particular with that examined by myself; when put into solution of ammonia it formed in a few hours the characteristic yellowish compound, identical in composition and properties with that already described.

The nature of the oil so far elucidated, it will be proper to return to the subject of the *amide*, or ammonia-compound, the latter term being however hardly applicable. The mode of preparation of this substance has been already described; it is always produced when ammonia and the oil are brought into contact. It is very pale yellow, approaching to white, and nearly inodorous when dry and pure; in cold water it is insoluble; alcohol and ether, on the other hand, dissolve it freely. It may be obtained in tufts of small, short, acicular crystals, by allowing a hot, saturated alcoholic solution to cool; or whiter and purer, by adding ammonia to a saturated aqueous solution of the oil, and allowing the mixture to stand several days. In chemical characters this amide much resembles the *hydrobenzamide* of M. LAURENT,

obtained by putting pure bitter almond oil into solution of ammonia. It is slowly decomposed by boiling water, and even by boiling alcohol, into free ammonia and volatile oil, and very slowly suffers the same kind of decomposition by moisture at the common temperature of the air. When heated, it melts, inflames, and burns with a smoky light, leaving a small deposit of charcoal. Acids decompose it immediately; a salt of ammonia is produced, and the original oil set free.

The action of alkalies on this substance is very remarkable, and well deserves attention. When boiled with a large quantity of dilute solution of caustic potash, it dissolves without the *least evolution of ammonia*, and the liquid deposits, on cooling, small, white, silky needles of a second new substance, having the *same composition* as the amide itself, but all the properties of a stable and exceedingly energetic organic base, possessing alkalinity, and having the power of forming with acids an extensive series of well-defined, and for the most part, crystallizable salts of great beauty.

Portions of the new salt-base, prepared at different operations with the greatest care, were submitted to ultimate analysis by burning with oxide of copper as before, the substance being dried *in vacuo* over a surface of oil of vitriol; the following results were obtained:—

	(1.)	(2.)	(3.)*
Substance employed . .	2·668 grs.	3·985 grs.	3·698 grs.
Carbonic acid produced .	6·53 grs.	9·74 grs.	9·06 grs.
Water produced . . .	1·12 grs.	1·63 grs.	1·51 grs.

Hence, in 100 parts,—

	(1.)	(2.)	(3.)
Carbon	66·75	66·66	66·82
Hydrogen . .	4·66	4·54	4·53

Estimation of nitrogen:—

	(1.)	(2.)
Substance employed. .	3·79 grs.	3·75 grs.
Platinum salt produced	6·12 grs.	6·11 grs.
Per-centage of nitrogen	10·18	10·28

The isomerism of the two substances is seen to be most complete; the numbers obtained by analysis absolutely coincide: and yet how discrepant their properties!

I am inclined to think that the nature of the isomeric change which the amide undergoes in presence of the alkali consists simply in a duplication of its elements; at least, this is the simplest view that can be taken. It is besides the only product. It will be seen from analyses of the salts formed by this substance, that the proportion of matter required to form a perfectly neutral compound with an acid, organic or inorganic, is expressed by the formula $C_{30}H_{12}N_2O_6$, which is the double of the *amide* $C_{15}H_6NO_3$. But then, as the constitution of this latter substance must be to some extent uncertain, since that of the oil is also unknown, and the *amide* forms no combinations, the above view must remain merely conjectural.

* This specimen was prepared from the oxalate by precipitation by ammonia.

I pass now to a description of the properties of the new salt-base, of its principal salts yet studied, and of the best method of preparation on a considerable scale.

The substance itself crystallizes from boiling water in fine, soft, white, silky needles, much resembling those of cafeine, quite permanent in the air, and even in the dry vacuum. It has but little taste, although its salts are very bitter; they are far less so however than those of morphia, or of the bark-alkalies. It is inodorous. At a temperature rather below the boiling-point of water it melts to a heavy, nearly colourless, oily liquid, which on cooling assumes at first the consistence of a soft resin, but eventually becomes brittle and crystalline. When strongly heated in the air, the new substance inflames, burns with a red and smoky light, and leaves but very little charcoal. It is soluble in about 135 parts of boiling water, but after cooling, scarcely a trace remains dissolved. Alcohol and ether in the cold dissolve it with the utmost facility; the alcoholic solution deposits, on spontaneous evaporation, exceedingly brilliant silky crystals; the liquid has a great tendency to creep up the sides of the vessel. The alkaline reaction to test-paper, when dissolved in hot water or alcohol, is exceedingly strong and well-marked. Dilute acids dissolve this substance with the utmost ease, becoming thereby completely neutralized, unless employed in excess; from these combinations the base is precipitated in an unchanged state, by the addition of ammonia or of a fixed alkali. A salt of the new base gives no precipitate with solutions of peroxide of iron, oxide of copper or silver, lime or baryta; the hydrochlorate forms with corrosive sublimate, a white, and with chloride of platinum, a bright yellow, double salt. What is rather remarkable, it is not precipitated to any extent by tincture of galls. So powerful are the basic properties of this alkaloid, that when boiled with a solution of sal-ammoniac, it decomposes that salt with evolution of ammonia and formation of a hydrochlorate.

The salts formed by this curious body are exceedingly numerous, and would in all probability well repay a more extended investigation; the few yet examined are the following:—

Hydrochlorate.—This is easily prepared by dissolving the alkaloid in dilute, warm hydrochloric acid, to saturation. The salt, which is perfectly neutral to test-paper, forms tufts of fine, silky, acicular crystals, like those of hydrochlorate of morphia. It is very soluble in pure water, but far less so in an excess of hydrochloric acid. The crystals retain their brilliancy when dried *in vacuo* over sulphuric acid. An analysis of this salt gave the following results:—

(Carbon and hydrogen.)	(1.)	(2.)	(3.)
Salt employed	5·855 grs.	5·435 grs.	6·115 grs.
Carbonic acid produced . .	12· grs.	11·12 grs.	12·55 grs.
Water produced	2·49 grs.	2·27 grs.	2·56 grs.

	(1.)	(2.)	(3.)
Carbon. . . .	55·89	55·62	55·97
Hydrogen. . .	4·72	4·64	4·65

6·027 grs. salt gave of chloride of silver 2·65 grs. = 10·67 per cent. chlorine.

6·68 grs. salt gave of chloride of silver 2·92 grs. = 10·60 per cent. chlorine.

6·147 grs. salt gave of double chloride of platinum and ammonium 8·24 grs.=8·45 per cent. nitrogen.

The formula $C_{30}\,H_{12}\,N_2\,O_6$, HCl+2HO, reckoned to 100 parts, gives numbers closely agreeing with the preceding; viz.—

Carbon	55·81
Hydrogen . . .	4·65
Nitrogen	8·72
Chlorine	10·98
Oxygen	19·84
	100·00

When a solution of the hydrochlorate of the new base is mixed with one of bichloride of platinum, an insoluble, or sparingly-soluble, bright yellow, crystalline precipitate falls, which is a compound of the two bodies. It is blackened and decomposed by boiling. When heated in the dry state, it melts, blackens, and swells up to a prodigious extent, evolving fumes of sal-ammoniac; the incineration of the charcoal is slow and difficult. Examined by analysis, the double salt gave the following results:—

Salt employed	7·28 grs.
Carbonic acid produced . .	10·16 grs.
Water produced	1·97 grs.

In 100 parts,—

Carbon.	38·06
Hydrogen.	3·

16·43 grs. salt gave of metallic platinum 3·36 grs., or 20·45 per cent.

The formula $C_{30}\,H_{12}\,N_2\,O_6$, HCl+$PtCl_2$, calculated to 100 parts, gives—

Carbon	37·97
Hydrogen	2·74
Platinum	20·90

Nitrate.—This is a very beautiful salt; it forms hard, transparent, colourless, and very brilliant crystals, whose form yet remains to be determined. It is freely soluble in pure water, but very sparingly soluble in excess of nitric acid. The crystals effloresce and become opake in a dry atmosphere. In this state they contain $C_{30}\,H_{12}\,N_2\,O_6$, NO_5+HO, as shown by the result of an analysis given below.

Nitrate employed . . .	5·62 grs.
Carbonic acid produced .	11·19 grs.
Water produced . . .	2·02 grs.

or, in 100 parts,—

Carbon	54·30
Hydrogen	3·99

The theoretical quantities are—

Carbon	54·35
Hydrogen	3·93

Oxalates.—The neutral oxalate is a very soluble salt, crystallizing, when the solution is sufficiently concentrated, in tufts of fine needles, like the hydrochlorate. The binoxalate is very sparingly soluble in the cold, and crystallizes remarkably well when a hot saturated solution is left slowly to cool. It forms transparent plates, like those of oxalate of urea, which have a strong acid reaction when dissolved, and retain their lustre in the dry vacuum. This substance gave on analysis the results stated below, leading to the formula $C_{30}H_{12}N_2O_6$, $2C_2O_3+2HO$.

Salt employed	4·808 grs.
Carbonic acid produced	10·05 grs.
Water produced	1·76 grs.

4·85 grs. salt gave of double chloride of platinum and ammonium 5·95 grs.; or 7·74 per cent. nitrogen.

Hence, in 100 parts,—

Carbon	57·01
Hydrogen	4·06
Nitrogen	7·74
Oxygen	31·19
	100·00

The theoretical numbers are—

Carbon	56·96
Hydrogen	3·91
Nitrogen	7·85
Oxygen	31·28
	100·00

The *acetate* is very soluble, and apparently uncrystallizable, or at least crystallizable with great difficulty.

The following is an excellent and easy method of preparing the vegeto-alkali in a state of purity and whiteness. The *amide*, dried in the air, or better, over oil of vitriol in the vacuum of the air-pump, is thrown into a large quantity of boiling-hot dilute solution of caustic potash contained in a capacious glass flask placed over a

lamp or chauffer. After ten or fifteen minutes' ebullition the change is complete, the great bulk of the new substance appearing in the form of a heavy, yellowish oil, which, on the removal of the vessel from the fire, collects at the bottom of the flask, and on cooling, solidifies, while that which had been dissolved by the liquid crystallizes out. When perfectly cold, the whole is thrown upon a cloth filter, slightly washed with cold water, and then dissolved in a large quantity of dilute boiling solution of oxalic acid, the acid being kept in considerable excess. The liquid, filtered hot, deposits on cooling a large crop of crystals of the acid oxalate of the base, dark-coloured, however, and impure. The salt may then be collected on a cloth filter, slightly washed and pressed, redissolved in boiling water, and heated for a few minutes with a little good animal charcoal, deprived of its earthy phosphates, &c., by washing with hydrochloric acid. The filtered solution now deposits the acid oxalate in a state of perfect whiteness and purity; from the pure salt the alkali may be obtained in crystals by solution in a large quantity of boiling water, addition of excess of ammonia, and rapid filtration at a high temperature. The crystals which form on cooling require of course washing with distilled water until all the ammoniacal and other salts are removed, and a portion of the alkaloid taken from the filter is found to leave no residue when completely burned on platinum.

I am in great doubt as to the most appropriate names to be bestowed on these curious bodies, and this doubt will remain until more is known respecting the real origin of the oil. This substance has no apparent connection with formic acid, except the accidental one of contemporaneous production. It is allied in *constitution* to the sugar and starch series, inasmuch as it contains oxygen and hydrogen in the proportions to form water, and in *properties* to bitter almond oil and the essential oils in general. So far as my own experiments go, it seems to be produced most freely and in greatest abundance from bran, 1 lb. of that substance distilled with half its weight of sulphuric acid and 3 lbs. of water having yielded nearly a drachm of oil, while mere indistinct traces could be obtained from similar quantities of rice- and potatoe-starch. Under these circumstances, perhaps the name "Furfurol" (from *furfur*, bran, and *oleum*) might be applied provisionally, and I am informed that this is the name which was proposed by the party who several years ago prepared a considerable quantity of the oil (a portion of which came into my hands, as mentioned at the commencement of this paper), and endeavoured to discover for it economical applications.

The following, therefore, will be the provisional nomenclature:—

Oil produced by the action of sulphuric acid on bran, &c., termed "furfurol," $C_{15} H_6 O_6$.

Product of the action of ammonia on furfurol, or "furfurolamide," $C_{15} H_6 NO_3$.

Vegeto-alkali, "furfurine," produced by the duplication of the elements of furfurolamide, $C_{30} H_{12} N_2 O_6$.

In conclusion, I beg to direct attention to the large and promising field of investigation offered by the study of the action of ammonia on the volatile oils, and on other allied bodies.

Middlesex Hospital,
Jan. 9, 1845.

NOTE ADDED DURING THE PRINTING.

Since the preceding paper was read I have received a communication from the gentleman just referred to, Mr. WILLIAM COLEY JONES, late of Plymouth, who has directed my attention to an article in the Polytechnic Journal, April 1840, in which he has described some of the more important properties of the oil in question, and among others, its power of generating a solid compound with ammonia, which when distilled with dilute sulphuric acid reproduced the furfurol. Mr. JONES prepared furfurol on a very extensive scale from the waste "lignin," or bran, separated from wheat in the process of starch-making; the details of the method however are not given, and there are no analyses of the products. I feel no hesitation in expressing my opinion that the honour of the independent discovery of this most curious body is justly due to Mr. JONES, and it is much to be regretted that it did not receive greater publicity.

Aug. 12, 1845.

IX. *On Benzoline, a new Organic Salt-base from Bitter Almond Oil.*

By GEORGE FOWNES, *Esq., Ph.D., F.R.S.*

Received May 9,—Read May 29, 1845.

WHEN pure oil of bitter almonds is left some days in contact with a strong solution of ammonia, at the ordinary temperature of the air, it is slowly, but in the end completely converted into a white crystalline substance, insoluble in water, but readily soluble in hot alcohol. The solidification of the oil is complete, and there is no secondary product. This substance was examined by M. LAURENT*, who conferred upon it the name of *hydrobenzamide*, and assigned to it the formula $C_{42}H_{18}N_2$; it is generated by the union of the elements of two equivalents of ammonia with those of three equivalents of hydruret of benzoyle, and the separation of six equivalents of water.

3 eq. bitter almond oil .	$C_{42}H_{18}O_6$	Hydrobenzamide	$C_{42}H_{18}N_2$
2 eq. ammonia.	H_6N_2	6 eq. water . . .	H_6O_6
	$C_{42}H_{24}N_2O_6$		$C_{42}H_{24}N_2O_6$

Acids decompose hydrobenzamide immediately, with separation of bitter almond oil and formation of salt of ammonia; with alkalies the case is different, solution of potash, even at a boiling heat, occasioning, as remarked by M. LAURENT, no perceptible change. I found however that when the boiling was prolonged for some hours, a change was induced resembling that undergone by *furfurolamide*† under similar circumstances. A few brownish crystalline flocks appear in the solution, and after cooling, the cake of resin-like substance is found harder and less fusible than hydrobenzamide which has been melted and left to solidify. This change is unaccompanied by any notable alteration of weight, although a faint odour of bitter almond oil is disengaged during the whole course of the ebullition. The new substance is an organic salt-base, having the same composition as hydrobenzamide itself; it might perhaps with propriety be called *benzoline*.

The salts formed by this substance are for the most part remarkable for sparing solubility, with the exception of the acetate; the hydrochlorate, the nitrate, and the sulphate are crystallizable; the last-named salt is exceedingly beautiful, crystallizing from an acid solution in colourless prisms resembling those of oxalic acid.

Precipitated by ammonia from a cold solution of the hydrochlorate or sulphate,

* Ann. Chim. et Phys. 62, p. 23.

† See preceding paper.

benzoline separates in white curdy masses, which when washed and dried diminished greatly in volume; when quite dry the powder is singularly electric; if rubbed with a spatula its particles repel each other with violence, scattering the powder over the paper on which it lies. It is not sensibly soluble in water, but dissolves with great ease in alcohol and ether. A hot alcoholic solution left for some time deposits the base in brilliant transparent colourless crystals, which apparently have the form of square prisms with variously-terminated summits; the alcoholic solution is strongly alkaline to test-paper. At a temperature below 212° benzoline melts, and on cooling assumes a transparent glassy state, without any tendency to crystallization. Heated in a retort, it boils and at length entirely volatilizes, with scarcely a residue of charcoal. Ammonia is disengaged during the distillation, a highly volatile oily liquid, having the odour of benzine, collects in the receiver, and a crystalline solid matter condenses in the neck of the retort. This latter substance, which seems to be the most abundant product, has been but partially examined; it is described below under the name *pyrobenzoline*.

The action of oxidizing agents upon benzoline is remarkable. When heated in a retort with a mixture of bichromate of potash, sulphuric acid and water, it is attacked with great energy, the mixture becomes dark green, and on distillation benzoic acid in large quantity passes over with the vapour of water. With nitric acid the same change seems to occur, but the action is not so definite and speedy. Hydrobenzamide, under similar circumstances, yields the same product, accompanied however in the first part of the distillation by a little bitter almond oil. Melted hydrate of potash appears to exert no action on benzoline, unless the temperature be excessive.

The composition of this substance and its isomerism with hydrobenzamide, are shown by the following analyses:—

	(1.)	(2.)	(3.)
Substance	4·018 grs.	4·03 grs.	4·57 grs.
Carbonic acid produced . .	12·37 grs.	12·46 grs.	14·14 grs.
Water produced	2·21 grs.	2·18 grs.	2·52 grs.

In 100 parts,—

	(1.)	(2.)	(3.)
Carbon . . .	83·96	84·32	84·38
Hydrogen . .	6·11	6·01	6·12

The nitrogen was determined by the process of MM. Will and Varrentrapp, as below:—

	(1.)	(2.)
Substance.	3·88 grs.	5·036 grs.
Platinum salt produced . . .	5·57 grs.	7·28 grs.
Per-centage of nitrogen . . .	9·07	9·12

Hydrobenzamide contains by calculation in 100 parts,—

Carbon	84·56
Hydrogen	6·04
Nitrogen	9·40
	100·00

Hydrochlorate.—The hydrochlorate of benzoline is a sparingly-soluble salt even in boiling water. It crystallizes from a hot solution in small but exceedingly brilliant colourless needles, which effloresce in the dry vacuum. It has, in common with the other salts, an intensely bitter taste. The salt, deprived of its water of crystallization, gave the following analytical results:—

	(1.)	(2.)
Substance	4·25 grs.	4·458 grs.
Carbonic acid produced	11·71 grs.	12·30 grs.
Water produced . . .	2·21 grs.	2·32 grs.

In 100 parts,—

	(1.)	(2.)
Carbon	75·14	75·25
Hydrogen	5·77	5·78

Estimation of nitrogen and chlorine:—

Substance	4·16 grs.
Platinum salt produced . . .	5·17 grs.
Per-centage of nitrogen . . .	7·83

	(1.)	(2.)
Substance	5·838 grs.	4·218 grs.
Chloride of silver produced	2·32 grs.	1·76 grs.
Per-centage of chlorine .	9·64	10·12

The formula $C_{42}H_{18}N_2$, HCl gives in 100 parts,—

Carbon	75·33
Hydrogen	5·68
Nitrogen	8·41
Chlorine	10·58
	100·00

The crystallized salt was found to lose by efflorescence 2·4 per cent. of water, corresponding very nearly to one equivalent.

The hydrochlorate of benzoline forms with bichloride of platinum an insoluble double salt of a pale yellow colour, not further examined.

Nitrate.—The nitrate is even less soluble than the preceding salt; the crystals are

small and have but little brilliancy; they are permanent in the dry vacuum. A portion subjected to analysis gave the following results :—

	(1.)	(2.)
Substance	4·112 grs.	4·27 grs.
Carbonic acid produced	10·51 grs.	10·91 grs.
Water produced . . .	1·97 grs.	2·03 grs.

In 100 parts,—

	(1.)	(2.)
Carbon	69·71	69·68
Hydrogen	5·32	5·28

The formula $C_{42}H_{18}N_2$, NO_5+HO, gives in 100 parts,—

Carbon	69·77
Hydrogen	5·26

The *acetate* is a very soluble salt; it dries up, on evaporation, to a gummy adhesive mass, and probably crystallizes with difficulty, if at all.

Pyrobenzoline.—This, as before observed, is the solid product of the dry distillation of benzoline. It is pressed between folds of bibulous paper to free it as much as possible from oily matter, and then crystallized from boiling alcohol, in which it dissolves pretty freely. It is but sparingly soluble in alcohol in the cold, and apparently quite insoluble in water, dilute acids and alkalies. It is tasteless, and the alcoholic solution has little or no alkaline reaction. At a high temperature this substance fuses, and with still further increase of heat distils; it sublimes at a temperature below its boiling-point, the vapour condensing in feathery crystals like those of benzoic acid. Melted pyrobenzoline on cooling forms a mass of radiated crystals, presenting a great contrast to the glassy or resinous appearance of benzoline under similar circumstances. This substance was at first imagined to be a hydrocarbon; it contains nitrogen, however, and gave on analysis the results stated below.

	(1.)	(2.)
Substance	4·295 grs.	3·46 grs.
Carbonic acid produced	13·42 grs.	10·74 grs.
Water produced . . .	2·08 grs.	1·71 grs.

Hence in 100 parts,—

	(1.)	(2.)
Carbon	85·21	84·66
Hydrogen	5·38	5·49

Estimation of nitrogen :—

Substance	4·15 grs.
Platinum salt produced . . .	6·00 grs.
Per-centage of nitrogen . . .	9·11

These numbers lead to the formula $C_{21} H_8 N$, which gives in 100 parts,—

Carbon	85·1
Hydrogen	5·4
Nitrogen	9·5
	100·00

This, which appears to be a neutral body, together with its accompanying liquid product, deserves a more extended examination.

The hope which I ventured to express in a former paper of the formation of new organic bases from the volatile oils which unite with ammonia, by subjecting their *amides* to the influence of agents, as caustic potash, capable of bringing about metamorphosis of the compound into a more stable form or forms of combination, has thus been partially fulfilled.

M. Laurent has recently announced the discovery of a new substance obtained from bitter almond oil, isomeric with hydrobenzamide, possessing basic properties, and corresponding in some other respects with benzoline; it is stated however to be volatile without decomposition, which is certainly not the case with that body. The name *amarine* was conferred upon it*. The publication of the experiments in detail will probably determine the identity or separate nature of the two substances.

* Comptes Rendus, xix. p. 353.

Middlesex Hospital,
May 8, 1845.

X. *On the Elliptic Polarization of Light by Reflexion from Metallic Surfaces.*

By the Rev. BADEN POWELL, *M.A., F.R.S., F.G.S., F.R.A.S., Savilian Professor of Geometry in the University of Oxford.*

Received April 24,—Read June 19, 1845.

IN a former paper, inserted in the Philosophical Transactions, 1843, Part I., I detailed observations on some phenomena of elliptic polarization by reflexion from certain metallic surfaces; but with reference only to one class of comparative results. From these I have been led to pursue the subject into other relations besides those at first contemplated; but, from various causes, have only been able at this interval to submit the results to the Royal Society as a sequel to my former observations.

The changes in the degree of ellipticity, investigated in my former paper, correspond to certain changes in the thickness of metallic *films*. If we now consider the case of reflexion from a *simple polished metallic surface*, and admit that in this case it may be supposed to take place by the penetration of the ray to a certain minute depth, or to some action of a thin transparent lamina of the metal, then, in like manner,—dependent on the law of metallic retardation,—the effect would vary with a difference in the *effective* thickness of the lamina, produced by changing the inclination of the incident ray; and that this is the case in general is well known, viz. that as the incidence is increased, the ellipticity increases up to a maximum, which occurs for most metals at an incidence between 70° and 80°, beyond which it decreases up to 90°.

The original researches of Sir D. BREWSTER*, to which we are indebted for the first investigation of these phenomena, afford a striking instance of the legitimate process of inductive inquiry in its first stage, in their total exemption from all reference to any physical theory. In these researches, besides the change in ellipticity with the incidence, there is also included the change in the virtual plane of polarization by metallic reflexion, which, though conjectured to be the same as that investigated by FRESNEL for transparent substances†, was only examined in detail at incidences at or near that for the maximum ellipticity, to which the author's object immediately restricted him. But for a series of metals *constant arcs* were accurately determined‡, which are the azimuths of polarization of the ray, restored to plane polarization after *two* reflexions from metal plates at the incidence for the maximum; while the ellipse, from which the appellation of "elliptic polarization" was derived, is a purely empirical representation of the varying arcs of incidence, considered as

* Philosophical Transactions, 1830, Part II. † Ibid. p. 292. ‡ Ibid. p. 294.

radii, for the second reflexion to restore plane polarization, at all azimuths of the plane of second reflexion to the first.

Though the subject of metallic reflexion is still in a condition of great obscurity, as to the mechanical causes to which its peculiar character is referrible, yet the application of the undulatory theory at least enables us to trace and connect some of its laws, and in the attempt to pursue such an application to some further relations, the nature of my researches may be briefly explained as follows :—

1. So far as the objects of my former inquiry were concerned, it sufficed to take the formula there employed for the polarized rings in the simplified form resulting from supposing a common coefficient to the two component vibrations; the plane of original polarization inclined 45° to that of reflexion, and 90° to that of analyzation. With reference however to some of the facts connected with observations at different incidences and azimuths of the polarizer, as well as on other grounds, it seemed desirable to generalize that formula by removing the above-mentioned restrictions; and I have accordingly here given an expression for the rings in elliptic light of all degrees with general coefficients, and for all positions of the polarizer and analyzer; which, though without difficulty deducible, has not, as far as I am aware, been stated by any writer.

2. With respect to the general character of *the rings*, the slightest observation shows that the distinction between the dark and bright centred systems in *plane* polarized light, though *modified*, is not *lost*, in the *lower* degrees of *ellipticity*; it disappears only when the light becomes perfectly circular; when the distinction is only seen in the changed direction of dislocation.

When the plane of analyzation is inclined 45° between the rectangular directions, and generally in intermediate positions, the whole appearance is, as it were, *distorted*; the dark arcs nearest the centre are situated towards *one end* of the quadrants, instead of being in the middle; and in the succeeding rings, though less strongly marked, there is an apparent increase of intensity towards the same *end* of the quadrant, owing to a general shade of darkness in the *ground* towards that side*. Of this appearance, though it must have been constantly seen, as far as I know, no explanation has been published. In *circular* polarization it does not occur. In plane polarized rings the analogous case is that of the well-known system of eight dislocated sectors; which in ellipticity of lower degrees is combined with, and passes into, that just described. All this is expressed by my formula.

3. The restoration of elliptic to plane polarized light by means of FRESNEL's rhomb, and the determination of the ellipticity by the azimuth of the rhomb, though an obvious process, yet has not, as far as I know, been pursued for any series of metals. Such a set of observations I have accordingly made at the incidence for the maximum ellipticity, for a considerable range of metals, some metallic ores, and other reflecting substances. Also in a few principal cases I have made similar observations at other incidences from 80° up to 30°, at which the ellipticity disappears.

* See Plate II. fig. 3.

4. In these cases the metallic reflexion performs the part of the first rhomb in FRESNEL'S experiment with two. If I rightly interpret Sir D. BREWSTER'S process for a similar restoration, before referred to, and if the two metal plates are analogous to the two rhombs, in the *change of plane*, which he so accurately determined, after the second reflexion, at the maximum incidence, we may infer a correspondence with that produced in the rhomb; and that it is *equal to twice the azimuth of the rhomb.* And in fact I find the results from two such different methods agree very closely for all the pure metals; though for certain ores, very low in the scale, there are some discrepancies.

There is also a close agreement between these results and the azimuths of the plane of the ray restored to plane polarization by the action of a crystallized plate, as given by the same author for some of the principal metals*, which confirms the same inference.

5. The *changes of plane* for successive incidences are in general of a nature *analogous* to those in the reflexion from transparent bodies, but not the *same.*

At the incidence for the maximum, if the plane of the polarizer be first adjusted to give, *e. g.* the dark system, then on changing its plane to 45°, the analyzer, in order to restore what is analogous to the same, that is, the *darkest* system, must coincide with the plane of incidence, or its azimuth is 0°, in exact agreement with what obtains both by theory and observation in the reflexion from transparent bodies.

At *greater* incidences, to restore the same system, the analyzer must be moved through increasing arcs on *one* side of the plane of incidence, and at *lesser* incidences on the *other*, as for transparent bodies.

Following up these latter, as the incidence is successively diminished, the azimuth of the analyzer changes in a manner obviously *different* from that which obtains for transparent bodies, though of the same *general* character: and the increase is slightly different for different metals; but in all cases it approaches 45° as the incidence approaches the perpendicular.

In these cases the azimuth of the polarizer remains at 45°. If it be changed, that of the analyzer preserves a constant relation.

Though these observations are of an obvious nature, yet, as far as I know, none of the kind have been published for any series of metals. I have accordingly given such a set for various reflecting substances.

But though apparently simple in principle, the process is troublesome in practice, and affected by various causes of uncertainty and difficulty. Some apparent anomalies, especially, which caused the arcs at small incidences to appear to increase beyond 45°, for a long time caused me much perplexity.

The results however here offered, having been obtained with the use of every precaution, and being the means of a great number of repetitions, I trust, on the whole, may not be useless as a first attempt to determine these changes by direct observation.

6. The ellipticity at different incidences cannot be deduced (except at the max-

* Philosophical Transactions, 1830, Part II. p. 311.

imum) from the rhomb observations without a knowledge of the change of plane: employing these latter data in combination with the former (3.), I have further estimated the ellipticity at different incidences for four principal metals.

7. For the application of the *undulatory theory* to these phenomena, we ought to be able to assign the law of metallic retardation, but this has not yet been done. The theory as here given indicates *the conditions of the maximum*, and shows in general *a change*, but not its *amount*.

Professor MacCullagh has however proposed, in accordance with a remarkable mathematical analogy, certain modifications of Fresnel's formula, which he has reduced to calculation in the case of *steel*.

My *theoretical* formula gives rise to an expression for the change of plane, but involving undetermined functions of the retardation. *On deducing the corresponding terms from Professor* MacCullagh's *data, and introducing them into my formula, I find it gives a very close representation of the observed results for steel.* Thus Professor MacCullagh's empirical expression receives an additional confirmation in accordance with a direct deduction from the undulatory theory.

The rest of this paper is devoted to the details of the observations, and of the analytical investigation.

Theoretical Investigation.

(1.) The original vibration in a plane P being

$$a \sin \frac{2\pi}{\lambda}(vt-x),$$

in general on reflexion in a plane R inclined to P by an angle ξ (R′ being the plane perpendicular to R) it is resolved into

$$a \cos \xi \sin \frac{2\pi}{\lambda}(vt-x) \ldots \text{in R},$$

$$a \sin \xi \sin \frac{2\pi}{\lambda}(vt-x) \ldots \text{in R}'.$$

(2.) But in the case of *metallic* reflexion, one of these components is accelerated in phase by a quantity ϱ, and at the same time for the greater generality, supposing the coefficients unequal, or changing a in the 2nd formula to b, and writing for brevity

$$\alpha = a \cos \xi, \quad \beta = b \sin \xi;$$

after metallic reflexion the component vibrations will be

$$\alpha \sin \left(\frac{2\pi}{\lambda}(vt-x)\right) \quad = \text{R},$$

$$\beta \sin \left(\frac{2\pi}{\lambda}(vt-x)+\varrho\right) = \text{R}'.$$

(3.) Here we may remark that these formulas give directly the equation to the elliptic vibrations, the ratio of whose axes is that of α to β, which vary at different incidences

as well as with changes given to ξ; or a and b are also functions of the incidence, or of ϱ. When $\varrho=\frac{\pi}{2}$ if $\alpha=\beta$ the light is circular; this is never the case in any metal at the maximum, though in some the ratio approaches it. For some other value of ϱ we may have $\alpha=\beta$, but this does not give circular polarization. When $\varrho=0$ the same formulas give the inclination of the plane of the rectilinear vibrations*.

(4.) On interposing a plate of crystal cut perpendicular to its axis, for any plane Q in the crystal passing through the axis, inclined to R by an angle φ, and to which Q′ is perpendicular, we have the vibrations

$$\text{R resolved into } \begin{cases} \text{R}\sin\varphi \text{ in Q}' \\ \text{R}\cos\varphi \text{ in Q}. \end{cases}$$

$$\text{R}' \text{ resolved into } \begin{cases} \text{R}'\cos\varphi \text{ in Q}' \\ \text{R}'\sin\varphi \text{ in Q}. \end{cases}$$

(5.) And on the general principle of resolution (observing that the resolved parts in one of the planes will be opposed), we have

$$\text{R}'\sin\varphi+\text{R}\cos\varphi=\text{Q}$$
$$\text{R}'\cos\varphi-\text{R}\sin\varphi=\text{Q}'.$$

The vibrations in Q′ form the ordinary ray O, and those in Q the extraordinary E. But after emergence the vibrations in Q are all further accelerated by θ, or become

$$\alpha\sin\left(\frac{2\pi}{\lambda}(vt-x)+\theta\right),$$

$$\beta\sin\left(\frac{2\pi}{\lambda}(vt-x)+\varrho+\theta\right).$$

(6.) Thus we have

$$\left.\begin{aligned} &\beta\sin\left(\tfrac{2\pi}{\lambda}(vt-x)+\varrho\right)\cos\varphi \\ -&\alpha\sin\left(\tfrac{2\pi}{\lambda}(vt-x)\quad\right)\sin\varphi \end{aligned}\right\}=\text{O}\ldots\text{ in Q}'.$$

$$\left.\begin{aligned} &\beta\sin\left(\tfrac{2\pi}{\lambda}(vt-x)+\varrho+\theta\right)\sin\varphi \\ +&\alpha\sin\left(\tfrac{2\pi}{\lambda}(vt-x)+\theta\quad\right)\cos\varphi \end{aligned}\right\}=\text{E}\ldots\text{ in Q}.$$

(7.) Now if the analyzer be applied with the plane of analyzation A, inclined to R by an angle χ, we shall have the angle $\text{AQ}=\psi=\chi-\varphi$, and $\text{AP}=\chi+\xi$.

The vibrations being again resolved in A and A′ perpendicular to it, we find

$$\text{O resolved into } \begin{cases} \text{O}\sin\psi \text{ in A}' \\ \text{O}\cos\psi \text{ in A}. \end{cases}$$

$$\text{E resolved into } \begin{cases} \text{E}\cos\psi \text{ in A}' \\ \text{E}\sin\psi \text{ in A}. \end{cases}$$

After analyzation the parts transmitted are those only in A, or

$$\text{O}\cos\psi-\text{E}\sin\psi.$$

* See my Treatise on the Undulatory Theory, &c., p. 12.

(8.) On substituting the values of O and E, and arranging the terms, this is reducible to the form

$$\mathrm{H}\sin\left(\frac{2\pi}{\lambda}(vt-x)\right)+\mathrm{K}\cos\left(\frac{2\pi}{\lambda}(vt-x)\right),$$

where

$$\left.\begin{aligned}&\beta\cos\varphi\cos\psi\cos\varrho-\alpha\sin\varphi\cos\psi\\&-\beta\sin\varphi\sin\psi\cos\varrho\cos\theta\\&+\beta\sin\varphi\sin\psi\sin\varrho\sin\theta\\&-\alpha\cos\varphi\sin\psi\cos\theta\end{aligned}\right\}=\mathrm{H},$$

and

$$\left.\begin{aligned}&\beta\cos\varphi\cos\psi\sin\varrho\\&-\beta\sin\varphi\sin\psi\sin\varrho\cos\theta\\&-\beta\sin\varphi\sin\psi\cos\varrho\sin\theta\\&-\alpha\cos\varphi\sin\psi\sin\theta\end{aligned}\right\}=\mathrm{K}.$$

Then, since the intensity I at any part of the image is expressed by

$$\mathrm{I}=\mathrm{H}^2+\mathrm{K}^2,$$

(9.) Squaring these quantities H and K, and taking the sum, after reduction, we ultimately find for any value of χ, or position of the analyzer,

$$\mathrm{I}=\left\{\begin{aligned}&(\beta^2\cos^2\varphi+\alpha^2\sin^2\varphi)\cos^2\psi\\&+(\beta^2\sin^2\varphi+\alpha^2\cos^2\varphi)\sin^2\psi\\&-\alpha\beta\sin 2\varphi\cos 2\psi\cos\varrho\\&-\alpha\beta\cos 2\varphi\sin 2\psi\cos\varrho\cos\theta\\&-\alpha\beta\sin 2\psi\sin\varrho\sin\theta.\end{aligned}\right.$$

(10.) Or, in order to see the consequences of changing the position of the analyzer, or the arc χ, we must introduce it by substituting for φ its value $\varphi=\chi-\psi$,

$$\mathrm{I}=\left\{\begin{aligned}&(\alpha^2\sin^2(\chi-\psi)+\beta^2\cos^2(\chi-\psi))\cos^2\psi\\&+(\alpha^2\cos^2(\chi-\psi)+\beta^2\sin^2(\chi-\psi))\sin^2\psi\\&-\alpha\beta\cos 2\psi\sin 2(\chi-\psi)\cos\varrho\\&-\alpha\beta\sin 2\psi\cos 2(\chi-\psi)\cos\varrho\cos\theta\\&-\alpha\beta\sin 2\psi\sin\varrho\sin\theta.\end{aligned}\right.$$

(11.) On expanding and reducing this becomes—

$$\mathrm{I}=\left\{\begin{aligned}&(\alpha^2\sin^2\chi+\beta^2\cos^2\chi)\cos^4\psi\quad\ldots\quad 1.\\&+(\alpha^2\sin^2\chi+\beta^2\cos^2\chi)\sin^4\psi\quad\ldots\quad 2.\\&+2(\alpha^2\cos^2\chi+\beta^2\sin^2\chi)\sin^2 2\psi\quad\ldots\quad 3.\\&-\frac{1}{2}(\alpha^2-\beta^2)\sin 2\chi\sin 2\psi\cos 2\psi\quad\ldots\quad 4.\\&-\alpha\beta\sin 2\chi\cos^2 2\psi\cos\varrho\quad\ldots\quad 5.\\&+\alpha\beta\cos 2\chi\sin 2\psi\cos 2\psi\cos\varrho\quad\ldots\quad 6.\\&-\alpha\beta\cos 2\chi\sin 2\psi\cos 2\psi\cos\varrho\cos\theta\quad\ldots\quad 7.\\&-\alpha\beta\sin 2\chi\sin^2 2\psi\cos\varrho\cos\theta\quad\ldots\quad 8.\\&-\alpha\beta\sin 2\psi\sin\varrho\sin\theta\quad\ldots\quad 9.\end{aligned}\right.$$

(12.) This formula is general for all positions of the polarizer and analyzer, and for light of all degrees of ellipticity. If we had taken (as in my former paper) $\psi=(45-\varphi)$, and at the same time supposed $\alpha=\beta=1$ (which is equivalent to the assumption then made for simplification), the formula (9.) would become at once the same as in that paper.

The first terms independent of θ express the intensity of the *ground* on which, as it were, the rings are formed: those involving ϱ vary with the incidence or degree of ellipticity: those involving θ give the rings; the last only being retained when the ellipticity is a maximum, and disappearing for plane polarization at the incidence 0.

From these formulas we at once trace all the well-known phenomena of the rings in plane and elliptic light, by following the changes in formula (9.) on advancing into the adjacent quadrants, or supposing ψ to become $\psi+90$, and by consequence changing also φ into $\varphi+90$; which gives a change of sign in the terms involving $\sin 2\psi$, $\cos 2\psi$, $\sin 2\varphi$, $\cos 2\varphi$.

Again, with regard to changes in the analyzer, it is evident that whatever be the value of χ if we increase it by 90°, we shall have to substitute in (10.) $\cos(\chi-\psi)$ for $\sin(\chi-\psi)$ and *vice versâ*; as well as $-\sin 2(\chi-\psi)$ for $\sin 2(\chi-\psi)$, and $-\cos 2(\chi-\psi)$ for $\cos 2(\chi-\psi)$, or, on the whole, the expression is obviously complementary, except the last term.

(13.) In any case on giving successive values to ψ round the rings, at those points where the sines or cosines vanish, the disappearance of any term when accompanied by a change of sign indicates (so far as that term affects the total intensity) a change from dark to bright at that point, or a complementary character in the adjacent portions of the image; when without a change of sign it indicates a simple maximum or minimum of light, in the rings or in the ground, according as the terms affected involve θ or not; and for light of different ellipticities according to the value of ϱ.

Thus for the values of χ in general, in (11.)—

At $\psi=0$, or 90°,

terms 1, 2, 3, for the ground, 8, for the rings } disappear without change of sign.

4, 6, for the ground, 7, 9, for the rings } disappear with change of sign.

At $\psi=45$,

5, for the ground, disappears without change of sign.

4, 6, for the ground, 7, for the rings } disappear with change of sign.

In *general* then there are dislocations of the *rings*, with more or less complete complementary changes in the *ground* both at the quadrants and half-quadrants.

At the maximum ellipticity, since terms 5, 6, 7, 8 disappear, there are dislocations only at $\psi=0$, with a change in the *ground* at $\psi=45$, or a gradual decrease of brightness from one *end* of the quadrant to the other, which is exactly the appearance observed. In plane polarized light we have the system of eight dislocated sectors,

intermediate between the dark and bright systems. In the lower degrees of ellipticity this is modified by, and passes into, that just described.

(14.) We may illustrate the application of the formula by one or two particular cases:—

1st. If we suppose at some incidence $\alpha=\beta$ while $\varrho<\frac{\pi}{2}$, then $\chi=45°$ will give branches with dislocation at $\psi=0°$, but none at $\psi=45°$; that is the nearest approach to the dark system. This agrees with formula (15.), where in this case I is a minimum when $\chi=45°$, also with (18.).

2ndly. On the same supposition $\chi=0$ will give complementary changes both in the ground and in the rings at $\psi=0$, and similar changes, though less conspicuous, at $\psi=45$; that is the intermediate system.

3rdly. For the general values of α and β at the maximum $\cos\varrho=0$; and $\chi=0$ gives branches with dislocation at $\psi=0$, but no change at $\psi=45°$; or the darkest system. But $\chi=45°$ will give (since $\alpha>\beta$) a complementary change in the ground at $\psi=45°$, and branches with dislocation at $\psi=0$; or the distorted system.

But if in this case $\alpha=\beta$, or the polarization be *circular*, the term (4.) disappears, and there is no distorted system in any position of χ.

Observation shows this to be the case in perfectly circular light, and very nearly so in the higher degrees of ellipticity.

(15.) For the branches, when $\psi=0$, for χ in general we have

$$I=\alpha^2\sin^2\chi+\beta^2\cos^2\chi-\alpha\beta\sin 2\chi\cos\varrho.$$

Hence, on making successively $\cos\varrho=1$, $\cos\varrho=0$, $\cos\varrho=-1$, &c., it is obvious that the intensity of the branches for the maximum ellipticity would be a mean between that in the dark and bright systems of plane polarized light if α and β were the same in the respective cases, which we shall see is the case; at all events, this relation of the intensities agrees with observation as far as the eye can judge.

(16.) Again, for the maximum ellipticity,

$$I=\beta^2+(\alpha^2-\beta^2)\sin 2\chi,$$

which can never be $=0$; or the branches are never absolutely dark; but it is evidently a minimum when $\chi=0$, and a maximum when $\chi=90°$, in which cases respectively $I=\beta^2$, or $I=\alpha^2$. If the polarization were circular these values would be equal, or the brightness the same in all positions of the analyzer.

(17.) For incidence 0°, the expression (15.) being made $=0$, or,

$$I=\alpha^2\sin^2\chi+\beta^2\cos^2\chi-2\alpha\beta\sin\chi\cos\chi=0,$$

we have for the position of the analyzer for absolutely dark branches, as in (3.),

$$\alpha\sin\chi-\beta\cos\chi=0,$$

or

$$\tan\chi=\frac{b}{a}\tan\xi.$$

But observation shows in this case that $\tan \chi = 45°$ when $\xi = 45°$ in all instances, hence for incidence 0° $a = b$.

(18.) More generally, differentiating (15.) in respect of χ, we find for any given value of ϱ, that is of α and β, the value of χ for the minimum, or darkest branches,

$$\tan 2\chi = -\frac{2\alpha\beta\cos\varrho}{\alpha^2-\beta^2},$$

or we might deduce directly

$$\tan\chi = -\frac{\alpha^2-\beta^2}{2\alpha\beta\cos\varrho} \pm \sqrt{\left(1+\frac{(\alpha^2-\beta^2)^2}{4\alpha^2\beta^2\cos^2\varrho}\right)}.$$

Hence we may make the same inferences as before.

And if when $\cos\varrho = 0$ we have also $\alpha = \beta$, or *circular* vibrations, it is worth while to observe that there results

$$\tan 2\chi = \frac{0}{0}.$$

But in general for the change of plane, or of χ, this formula does not assign any precise values, since the form of the function of ϱ involved in α and β is unknown.

(19.) In the absence of any *theoretical* law I have had recourse to the *empirical* modification of FRESNEL's formula proposed by Professor MACCULLAGH*. And availing myself of the computed values which he has given, in the case of steel, for the coefficients of the component vibrations $\alpha\ \alpha'$ (assuming $\alpha' = \beta$ in my notation) and the retardations $\delta\ \delta'$ (where $\delta - \delta' = \varrho$ in my notation), I have deduced the values of the product $\alpha\ \alpha'$, of $\alpha^2 - \alpha'^2$, and of $\cos\varrho$, and introducing these in formula (18.), I find the resulting values of χ in close agreement with those given by observation. The elements of this computation and its results are exhibited in Table IV.

APPARATUS.

The general principle of my apparatus will be apparent from the nature of the experiments, but it may not be superfluous (especially for those who may wish to pursue similar observations) to annex an outline of the construction, which, after trial of various forms, I have found best to unite the requisite conditions for the purpose in view; while it is readily convertible into an ordinary polariscope; besides admitting of the addition or substitution of other parts, when required, for different objects.

The accompanying Plate II. fig. 1 gives a general view of the arrangement and will sufficiently explain itself: the polarizing part (P) contains a NICOL prism which can be turned in azimuth, measured by a graduated circle; a condensing lens (L) is also attached to it.

The analyzing part (A) contains in the eye-piece (E) a lens, a calc-spar, and a tourmaline; and bears a graduated circle, on which the azimuth of analyzation is read off.

The metal under examination is placed on the support (R), which can be raised or

* Reports, Royal Irish Academy, October 1836.

lowered as required; and should be capable of a slight inclination for better adjustment.

The middle part or hinge (M) is surrounded by a graduated vertical circle, by which the parts (P) and (A) can be set at any angle to each other: they are attached to it by projecting arms: the whole is moved by the joint (K) till the reflected light from (R) comes distinctly to the eye through (E), when (it is easily seen) the angle of incidence will be *half* that measured on (M).

The zero of (M) is found by adjusting (P) and (A) with their axes in one line (or reducing the instrument to an ordinary polariscope). The hinge should be capable of being firmly clamped.

In observations at very great incidences it will be easily seen to be necessary to have the arms carrying (P) and (A) so fixed, that when the axes of the tubes are brought into one line there shall be a considerable space between their inner ends: there should be also a slight motion about the axis of the arm for adjustment in (P).

In observations at very small incidences, it is most convenient to throw the light on to (P) by means of a small mirror placed in a proper position.

Fig. 2 represents the mode of applying the FRESNEL rhomb (F) to the analyzing part, the rest of the arrangement remaining the same. The eye-piece (E) is removed to a cap on the top of the rhomb; while the bottom of the rhomb is attached to a short tube, which fits into the aperture before occupied by the eye-piece.

Fig. 3 represents the appearance of the rings in elliptic light when the analyzer is at 45° between the dark and light systems.

OBSERVATIONS.

The determination of the precise position of the analyzer at which the maximum or minimum brightness is attained, is from the nature of the case open to considerable uncertainty. But the intervention of the *rings*, instead of using the simple analyzer, affords some aid in this respect, since the change in the nature and form of the rings offers a more ready guide to the eye; especially in elliptic light, where there is never a total evanescence. Still the undefined nature of the object observed precludes minute numerical accuracy, and the results in the following tables must be regarded as no more than approximations, though derived from the means of a great number of repetitions. In taking these means, I have omitted fractions of a degree as bearing an appearance of accuracy quite illusory.

The greatest care is necessary in the adjustments, especially of the polarizer. They were remade for each set of observations. That for the polarizer to obtain the zero or coincidence with the plane of reflexion, was found by varying the azimuth till the light was perfectly restored to plane polarization; the analyzer being in the position for dark branches, the zero of its circle was determined at the same time.

In the observations at different incidences, in order to secure the constancy of its inclination to the plane of incidence, the polarizer was adjusted to zero before obser-

vation at each incidence. For some of the ores, &c. which reflect but little light, the arcs must be taken as mere estimations. I have found some peculiar discordances between different sets of observations, especially in silver and copper: those given are the means.

The observations here annexed in a tabular form consist of,—

1. Those with FRESNEL's rhomb for ascertaining from the azimuth (γ) for the restoration of plane polarization, the ellipticity at the incidence for the maximum; or giving

$$\varepsilon = \left(\frac{\beta}{\alpha}\right)_m = \tan \gamma_m.$$

These results for the principal metals and other substances are seen in Table I., columns 3 and 4.

Again, in the observations with the rhomb at azimuth γ, the plane of the emergent ray is inclined by 2γ to the original plane. In Sir D. BREWSTER's experiments with two metal plates it is inclined $45+\varphi$. The results are here compared in columns 1 and 2, and sufficiently show that we have

$$\gamma = \frac{45+\varphi}{2}$$

for all the pure metals.

Also we may remark, that Sir D. BREWSTER's empirical ellipse has the ratio of its axes determined by $\sin 2\varphi$, and is therefore different from the undulatory.

The same author also gives* the azimuths of the plane of the ray restored to plane polarization by a crystallized plate for several metals, as follows:—

Pure silver . . .	42°	Speculum . . .	32°
Copper	36° 30′	Steel	30° 30′
Mercury	35°	Lead	26°
Platinum	34°	Galena	17° 30′

In each instance it will be seen (on comparison with Table I.), that as nearly as possible the arc $= \frac{45+\varphi}{2}$; with the exception of galena.

2. Observations for the values of χ at successive incidences, while ξ remains at 45°.

These series of arcs are given in Table II., column 1, for the two metals of greatest and least maximum ellipticity and two intermediate; and in Table III. for a number of others.

3. Observations for the ellipticity at different incidences.

In general the ellipticity is measured by the inclination of the rhomb (γ) to the plane of previous polarization.

For the maximum ellipticity, since $\chi=0$, it follows that γ is correctly measured from the plane of reflexion. But at *other* incidences γ must be corrected by the change of plane of polarization due to that incidence, or we must take $(\gamma-\chi)$.

In Table II. (columns 2 and 3) are given the observed values of γ for four principal

* Philosophical Transactions, 1830, Part II. p. 311.

metals at different incidences, and the corresponding ellipticities (ε) or tan ($\gamma-\chi$). These results give on the whole a view of the changes of ellipticity agreeing with the general appearance of the rings; though in copper the diminution appears too rapid.

TABLE I.

Metal.	Values of φ (BREWSTER).	$\frac{45+\varphi}{2}$.	Azimuth of rhomb $=\gamma$.	Ellipticity $\varepsilon=\tan\gamma$.
Silver, pure	39° 48′	42° 24′	42°	·932
Silver, common	36	40 30	40	·839
Gold, pure	35	40		
Gold, common	33	39	40	·839
Brass	32	38 30	38	·781
Copper	29	37	38	·781
Mercury	26	35 30	35	·700
Platinum	22	33 30	34	·674
Palladium	....	....	34	·674
Speculum	21	33	31	·600
Bismuth	21	33	34	·674
Zinc	19 10	32 5	32	·624
Steel	17	31	30	·577
Iron	....	....	29	·554
Antimony	16 15	30 37	31	·600
Lead	11	28	28	·531
Tinned plate	....	....	32	·624
Plated copper	....	....	32	·624
Iron pyrites	14	29 30	29	·554
Galena	2	23 30	24	·466
Specular iron ore	0	22 30	24	·466
Tempered steel, yellow	....	....	20	·363
Tempered steel, blue	....	....	15	·267
Plumbago	....	....	15	·267
Decomposed glass	....	....	7	·122

TABLE II.

Pure silver ($\xi=45°$).					Copper ($\xi=45°$).			
Observed values of					Observed values of			
i.	χ.	γ.	$\gamma-\chi$.	ε.	χ.	γ.	$\gamma-\chi$.	ε.
30	42	45	3	·052	44	45	1	·017
40	36	45	9	·158	40	45	2	·034
50	31	45	14	·249	23	44	11	·194
60	25	44	19	·344	24	43	19	·344
70	10	43	34	·674	10	39	29	·554
	73) 0	42	42	·932	73) 0	38	38	·781
80	−25	45	20	·363	−21	40	19	·344
Steel ($\xi=45°$).					Lead ($\xi=45°$).			
Observed values of					Observed values of			
i.	χ.	γ.	$\gamma-\chi$.	ε.	χ.	γ.	$\gamma-\chi$.	ε.
30	43	45	2	·034	45	45	0	·0
40	41	45	4	·069	42	45	3	·052
50	34	43	9	·158	30	43	13	·230
60	29	41	13	·230	24	42	18	·324
70	15	35	20	363	8	33	25	·466
	75) 0	30	30	577	72) 0	28	28	·531
80	−19	34	15	·267	−14	35	21	·383

Table III.

i.	Observed values of χ. $\xi=45°$.							
	Gold.	Brass.	Mercury.	Platinum. Palladium.	Bismuth.	Zinc.	Antimony.	Speculum.
30	44	43	45	44	43	44	44	44
40	40	42	44	43	42	43	43	42
50	36	39	42	40	41	41	42	40
60	17	35	39	38	36	39	38	32
70	0	15	29	32	23	25	25	17
		72) 0	78) 0	78) 0	74) 0	77) 0	75) 0	76) 0
80	−30	−19	− 7	−10	−25	−10	−20	−20

i.	Observed values of χ. $\xi=45°$.							
	Plated copper.	Tinned plate.	Tempered steel, yellow.	Tempered steel, blue.	Decomposed glass.	Specular iron ore.	Galena.	Plumbago.
30	43	44	43	43	43	43	44	45
40	41	43	40	41	42	42	43	44
50	40	41	26	35	38	35	40	35
60	37	40	0	10	9	27	30	24
				62) 0	62) 0			
70	20	22	−25	−21	−35	17	15	10
	72) 0	77) 0	....	....	....	77) 0	72) 0	75) 0
80	−30	−15	−45	−44	−44	−10	−20	−15

Table IV.

i.	Steel.				
	From MacCullagh's data.			Values of χ.	
	$\alpha\alpha'$.	$\alpha^2-\alpha'^2$.	$\varrho=\vartheta-\vartheta'$.	Calculated by (18).	Observed.
°			°	° ′	°
0	·526	0·0	0	45	nearest 0° 45
30	·522	·100	8	42 14	43
45	·408	·231	19	36 40	36
60	·474	·421	41	29 45	29
75	·451	·610	90	0	0
85	·682	·456	150	−28 5	−27
90	1·0	0·0	180	−45	nearest 90° −43

Postscript.

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Since the foregoing paper was communicated I have obtained some additional instances of elliptic polarization, which may be not uninteresting in connexion with those therein mentioned.

Besides plumbago (in which the proportion of metal present is at least very variable and small) I have now observed a slight, but quite unequivocal ellipticity in the reflexion from *China ink*, which is wholly non-metallic. It is at its maximum at an incidence of 62°, and its azimuth of restoration by the rhomb is about 5°.

I have examined also *chromate of lead*, which gives very small ellipticity, which is at a maximum at an incidence of 70° (agreeing well with the mean refractive index), and the azimuth of the rhomb is about 6°.

XI. *Electro-Physiological Researches.—First Memoir. The Muscular Current.*

By Signor CARLO MATTEUCCI, *Professor in the University of Pisa, &c. &c.*
Communicated by MICHAEL FARADAY, *Esq., F.R.S., &c. &c.*

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MY only resource for showing the Royal Society how grateful I feel for the distinction lately accorded to me, is the communication of some fresh researches on electro-physiological phenomena.

The exposition of these researches will form the subject of the present and subsequent memoirs.

From the commencement of my studies on this subject, my principal aim has always been to reduce the experiments of electro-physiology to the simplest possible form, so that they may be repeated without the aid of very expensive instruments, or such as require great skill and practice in the management.

It is for this reason that I have dwelt long upon the phenomena which the electric current occasions in its passage along the nerves of an animal recently killed. The galvanoscopic frog, the mode of preparing which, together with its use and all its details, I have described in my Traité des Phénomènes Electro-Physiologiques des Animaux, page 51, is indubitably a very delicate galvanoscope, and free from all error. By means of the galvanoscopic frog properly applied, it is easy to ascertain the direction of the current which traverses the nervous filament of the frog itself. There is only this to be observed, that it is essential to the occurrence of this indication to wait until the frog be sufficiently weakened; and that in spite of this precaution, in every series of experiments we find some one frog in which, although we always have the signs of the electric current, yet contraction fails to take place when the circuit is closed with the direct current, and when it is broken with the inverse.

A new method of employing the frog, which I shall presently describe, adapts itself better than the galvanoscopic frog to the demonstration of the existence and direction of the muscular current, and of the proper current of the frog, and therefore supersedes the necessity of a galvanometer. For this purpose the frog is prepared in the ordinary manner of GALVANI, that is to say, it is cut in half through the middle of the vertebral column, skinned, and the viscera removed. It is then easy, with the help of scissors (introducing them under the lumbar plexuses), to remove the greater part of the pelvis of the frog, leaving the above-mentioned plexuses intact;

finally, the frog is divided into two parts by cutting the juncture of the two thigh-bones. In this way there remain two halves of the frog united together organically by their spinal nerves. When this frog is required to be used for the purpose of discovering the presence and direction of the muscular current, it should be disposed upon an insulating plane in such a manner that its two extremities or claws dip into two separate recipients. Joining these two recipients with strings of cotton or thread, soaked with the same liquid as that contained in the recipients themselves, or with a strip of paper similarly wetted, no sign of contraction is ever produced, therefore no current is in circulation. This last fact may easily be proved by closing the circuit with the two ends of the platinum wire of a delicate galvanometer, provided we can be perfectly certain of the homogeneity of the substance of the extremities of the wires. And it will be seen that it cannot be otherwise, on reflecting that each half of the frog constitutes an electro-motor element of the proper current, so that, in the above-described manner of using the prepared frog, there are always two equal currents circulating in contrary directions, and which consequently neither excite contractions, nor deflect the needle of the galvanometer.

There is nothing easier, and at the same time more decisive, than the confirmation of the existence of the muscular current and of its direction.

A pile consisting of thighs of frogs, or of muscles of other animals, should be prepared in the manner I have described in my work that I have quoted above. The two extremities of this pile (the internal surface of the muscle on the one hand, and the external on the other) should dip in distilled or spring water. When the frog is prepared as already described, and stretched upon the insulating plane, the extreme cavities of the pile are made to communicate with the two recipients in which the claws of the frog are immersed, by means of strings of thread or cotton soaked with water (Plate III. fig. 1.). The frog is then distinctly seen to contract both on closing and on breaking the circuit; but both limbs do not contract equally, since that which is traversed by the current, and which consequently is near that extremity of the pile which is formed by the internal surface of the muscle, contracts on closing the circuit with the direct current, while, on the contrary, the other limb, which is near the extremity of the pile formed by the external surface of the muscle, contracts on breaking the circuit. Simply by the aid of the frog so prepared, it is possible to confirm the principal laws of the muscular current, which I have already discovered with the galvanometer. Thus it happens that the contractions of the galvanoscopic frog increase proportionally with the number of elements; and they are the same for a pile formed of muscular elements deprived of all visible nervous filaments, as for a pile the muscular elements of which are intact. The same takes place operating with a pile composed of muscular elements taken from frogs killed by the action of narcotic poisons, carbonic acid, prussic acid, &c. Finally, with the galvanoscopic frog it is easy to discover the immense difference which exists between the signs of

the muscular current, occurring some time after death in piles composed of the same number of muscular elements, but taken from different animals, as frogs, eels, or pigeons. The signs of the current diminish in a rapid ratio as we ascend in the scale of animals upon which we operate.

Hitherto I have merely exhibited experiments which confirm my former conclusions. I have simply sought to show how that without a very delicate galvanoscope, and with the galvanoscopic frog alone, any person may be enabled to demonstrate the principal laws of the muscular current.

It has always been an object of peculiar interest to me, in the study of the muscular current, to establish fresh experimental proof, or to repeat and extend my former ones; all which have led me to conclude that the existence and intensity of the muscular current depend upon the existence and intensity of the changes of structure and composition, which constitute the nutrition of the muscle.

Nevertheless, before entering upon the exposition of my researches instituted in this view, I will describe some fresh facts, which, although they may be anticipated, notwithstanding, better serve to fix the origin of the muscular current.

With a pile of half thighs of frogs I have distinctly obtained the decomposition of iodide of potassium. To effect this, I soak a piece of paper in a solution of this substance, and upon it, about the distance of a line apart, I place the extremities of two platinum wires, the other ends of which dip in the fresh water which fills the extreme cavities of the pile. To facilitate and expedite the decomposition of the hydruret, I increase the extent of those portions of the platinum wires which are immersed in the cavities of the pile, by twisting the extremities into a coil; I am also in the habit of further wetting the paper soaked with the solution of hydruret of potassium, with a solution of starch paste, to which I add a few drops of chlorine. After the circuit has remained closed a few seconds, a blue or a yellow spot is formed around the wire which communicates with the cavity in which the external surface of the muscle is immersed.

With a pile of twenty elements of half thighs of frogs, I have likewise distinctly obtained signs of tension, by means of a tolerably delicate condenser. To this intent I put one extremity of the pile in communication with the ground, and the other with the plate of the condenser. I have frequently repeated the experiment, at one time establishing a communication between the internal surface of the muscle and the condenser, and the external surface with the ground; at another time I have reversed this order. I have likewise observed the phenomena which ensue on putting each of the extremities of the muscular pile in communication with one of the plates of the condenser. In every case the electroscope has constantly exhibited signs of a negative charge upon the internal surface of the muscle, and of a positive charge upon the external surface.

Again, I was desirous of ascertaining how the signs of the muscular current were affected on excluding the air from contact with the pile. For this purpose I em-

ployed the receiver commonly in use for the experiments of electricity *in vacuo*, or in other gases than air. The two platinum wires are fastened to the metallic rod which moves in the tube in the centre of the receiver; one of the wires, however, is insulated from the rod by a layer of gum-lac which intervenes where the wire encircles the rod. These two wires are twisted into the shape of a pitchfork, and so diverge from one another, that on pushing the rod down the two extremities enter the extreme cavities of the muscular pile (fig. 2.). I rapidly exhausted the air in the receiver, and then connected the platinum wires with the galvanometer. I have repeated the experiment while the air was exhausted, and immediately after admitting the air again into the receiver. The direction of the current has never varied, nor have I perceived any very great difference in the intensity and duration of the current in the different modes of conducting the experiment.

It will be readily understood from the description of the apparatus employed for operating in a vacuum, how easy it was to adapt it to experiments in other gases: I have used hydrogen and carbonic acid.

I think it important to describe minutely the results obtained in these different experiments. I began with a pile composed of twenty muscular elements, or half thighs of frogs, placing it under the receiver, and I filled the cavities of the pile with well-water, after ascertaining that there was no sign of a current on immersing the ends of the wire in the same liquid. In less than five minutes the frogs were killed and the pile prepared. In performing these comparative experiments, I have made use of frogs caught on the same day and in the same pool. In all these experiments the circuit was kept closed, and the duration of the current in the galvanometer observed for several hours successively. The following are the numbers obtained in the different experiments.

Operating in atmospheric air with a pile of twenty elements of frogs, the deflection in my galvanometer was so strong that the needle reached 90°, oscillated, then gradually returned towards 0°. At about 30° the needle begins to be stationary, or at least it retrogrades considerably more slowly than at first. Every ten minutes I noted down the deviations, which are the following: 15°, 9°, 5°, 4°, $3\frac{1}{2}$°, $3\frac{1}{2}$°, $3\frac{1}{2}$°, $3\frac{1}{2}$°. At the expiration of two hours the deflection was the same. The circuit was then broken, and the extremities of the wires immersed in pure water, the current produced was 70°, and its direction was contrary to the first when the circuit was closed with the muscular current. This was evidently the secondary current which at the commencement of my researches I showed to be the cause of the rapid decline of the muscular and proper current in those cases where the circuit is kept closed. I have twice repeated this same experiment with the same results.

I have constructed a third pile similar to the two already described, which I have put under the receiver mentioned above, and have exhausted the air to that degree of rarefaction that the mercurial column showed only one inch of pressure. I could not attain to a more complete degree of rarefaction on account of the aqueous vapour

which was continuously formed. I then closed the circuit, and the deflection at first was 85°; as usual the needle oscillated, retrograded, and began to be stationary towards 30°. The following numbers show the degree of deflection at intervals of ten minutes, care having been taken to maintain constantly the degree of rarefaction unaltered. After the first ten minutes the deviation was 15°, 10°, 7°, 5°, 5°, 4°, after two hours the deflection was fully 3°. On admitting the air into the receiver the needle advanced two or three degrees, then again returned to its deflection of 3°. This slight increase in the strength of the current occurs, as we shall presently see, whatever be the gas introduced into the receiver; it is also caused by working the air-pump while the circuit is closed, and this is probably owing to the liquid contained in the cavities of the pile being put in motion, so that the wires are more or less immersed. The lesser deviation obtained in this third experiment on first closing the circuit was very probably owing to the frog having been killed rather longer than the others, to the muscular elements immediately beginning to dry in rarefied air, and to some bubbles of air which adhere to the platinum wires when the pump is worked.

I next killed forty other frogs, and with them I composed two piles consisting of twenty elements each pile. I tried the effect of each of these piles separately in contact with the air; in one of them the first deflection was 85°, in the other 88°. I then put one of these piles under the receiver and exhausted the air; I closed the circuit, and the deflection was 81°. The needle oscillated, retrograded as usual, and remained fixed at 15° at the expiration of ten minutes. I now introduced a sufficient quantity of oxygen gas to fill the receiver. The needle oscillated slightly on admitting the gas, then continued retrograding, and after a lapse of thirty minutes it marked 8°, and at the end of another half-hour it showed 4°.

After this I placed the other pile under the receiver, exhausted the air, and closed the circuit. The first deviation of the needle was 65°. This difference is naturally owing to the length of time the frogs had been killed, that is, all the time taken up by my first experiment. Leaving the circuit closed, the needle as usual continued to retrograde, becoming stationary at 4°, as did the other pile immersed in oxygen. Finally, I re-admitted the air, and the deflection was not increased.

These experiments are sufficient to show that the muscular current neither varies in intensity nor in duration, after the death of the animal, from keeping the muscles from which it is obtained either in oxygen or in air reduced to a pressure equivalent to one inch of mercury.

I will now describe the results obtained from operating in the manner above described, in hydrogen gas. The singularity which this gas offers could not certainly have been anticipated before the experiment.

As usual I prepared a pile of twenty elements, half thighs of frogs; I placed it under the receiver, and previous to exhausting the air I completed the circle, and the deflection was 85°: the circuit was left closed. I then rapidly exhausted the air

and substituted hydrogen gas in its place. After the circuit had been closed ten minutes, the needle of the galvanometer remained stationary at 15°. But then instead of continuing to retrograde it began to advance, so that the deflection was 50° at the end of ten minutes. I broke the circle by drawing the rod up, and when the needle had returned to 0° I again completed the circuit; the needle now advanced to 90° in the usual direction of the muscular current, and remained fixed at 55°. From this point the needle of the galvanometer descended very slowly, being at 40° at the end of an hour. I then exhausted the hydrogen and re-admitted air, and the needle returned to 12°, continuing to recoil, but still more slowly than in the other experiments.

I repeated this same experiment with precisely the same results; that is to say, the needle descended as usual before the hydrogen was let into the receiver; after the introduction of the gas it advanced to 50°. I then produced a vacuum and again admitted the air, and the needle again descended, still however keeping up a greater deflection than there would have been if the hydrogen had never been used. I have been able, several times with the same pile, to observe the alternations of the effects produced by air and by hydrogen gas. Prolonging the contact of the hydrogen for some time, I have constantly observed that when that gas was exhausted and atmospheric air substituted for it, the final deflection of the needle, taking into account the time elapsed since the preparation of the pile, and *cæteris paribus*, was much greater than it would have been if no hydrogen had been introduced into the receiver. Finally, I will mention another experiment performed by closing the circuit with the usual muscular pile after having filled the recipient with hydrogen. The first deflection was the usual one, and then followed 47°, 41°, 40°, 38°, 37°, 35°, 34°, 32°, 31°, 30°. These signs of deflection were noted down at intervals of ten minutes. After six hours the needle was stationary at 25°. When hydrogen is not used, that is in atmospheric air, *in vacuo*, or in oxygen, the deflection does not exceed 5° with the usual pile of twenty elements, after the circuit has remained closed an hour.

Although I could not attribute this singular difference produced by the presence of hydrogen upon the muscular pile to the action of this gas upon the source of electricity in the muscles, nevertheless it appeared to me important to discover the cause of this difference. I very quickly prepared forty elements (half thighs of frogs), using every possible precaution to have all those circumstances equal in all the conditions which are known to influence the muscular current. Thus, two individuals prepare the frogs at the same time, each frog is divided into halves, and two separate heaps are made, from each of which twenty elements, or half thighs, are selected. I leave twenty of these elements exposed to the air, and the others in an atmosphere of hydrogen by means of the receiver.

At the expiration of forty minutes I take these twenty elements out of the hydrogen, and compose the pile, and I do the same with the other elements left in contact with

the atmosphere. I oppose the two piles to one another in the manner I have described in my work quoted above, for the purpose of discovering the differential current. This done, I determine the intensity of the muscular current in each of the piles, taking note of the first deflection, and of that which the needle indicates after the circuit has been closed ten minutes. In another experiment performed in exactly the same manner, I confronted the current of one muscular pile, the elements of which had been in hydrogen, with another, the elements of which had been in air highly rarefied. Finally, before putting the muscular elements in hydrogen gas and in rarefied air, I measured the muscular currents of each of these piles, in order to compare them with those which were to follow. I should be too long if I were to report all the numbers of the different experiments which I performed. The conclusion to be drawn from them all is extremely simple. Hydrogen gas does not act differently upon the muscular elements from oxygen, and atmospheric or rarefied air, or in other words, these different gaseous media do not exert any influence upon the intensity and duration of the muscular current.

It only remained for me to determine precisely the cause of the singular effect exhibited by hydrogen; and as I could not doubt that this effect was owing to the action of hydrogen gas upon the secondary polarities evolved upon the platinum, I tried the following experiment. I introduced a platinum wire into a tube of glass, and with the blow-pipe I soldered it to the upper and closed end of the tube. I constructed a pile of twenty elements, and closed the circuit, as seen in fig. 4. The tube filled with water is inverted in the liquid contained in the extreme cavity of the pile, in which the outer surface of the muscle is immersed. When the circuit was closed the first deflection was 90°, then the needle retrograded as usual. When the deflection was at 20°, I introduced hydrogen into the tube A, and the needle instantly began to rise to 25°, 30°, 40°, 50°. I allowed the hydrogen to escape, filling the tube again with water and closing the circuit, and after ten minutes the needle was at 5°. Again, I filled the tube with hydrogen and the needle rose to 20°, 25°, 30°. Finally, I transferred the hydrogen tube to the other cavity in which the inner surface of the muscle was immersed, and instead of advancing the needle only fell more rapidly.

The effect then of the hydrogen gas is to act upon the oxygen which tends to be evolved upon the platinum which transmits the muscular current, which passes, as is well known in the muscular element itself, from the interior of the muscle to the surface. It is a case analogous to the gas-pile of GROVE. These researches seem to prove that the cause of the rapid diminution, both of the muscular and proper current, from the circuit being kept closed, lies in the secondary polarities of the platinum extremities, which generate a current that circulates in a direction contrary to that of the muscular pile. I have yet to mention the experiments attempted by putting the muscular elements in contact with carbonic acid for different lengths of time. I have performed three experiments of this nature, in each of which I have confronted

the pile, the muscular elements of which had been in carbonic acid, with another pile similar to the former, but which had merely been left in contact with the air. After having opposed the two piles to one another, I have closed the circuit without ever having found that any difference was induced by the carbonic acid.

Summing up the results of the various and repeated experiments attempted in the view of discovering the influence of some gaseous media (air, air greatly rarefied, oxygen, hydrogen and carbonic acid) upon the intensity and duration of the muscular current, we must conclude that this influence is either null or insensible, or in other words, that the immediate causes of the development of electricity in the muscles reside in the muscular substance, independently of the gaseous medium in which it is placed.

In order better to determine the relation between the signs of the muscular current and the organico-vital conditions of the muscle, since the last few months of the past year I have directed my attention towards ascertaining the intensity of the muscular current generated by a pile of half thighs of frogs, always made up of twenty elements. I did this for the purpose of comparing the influence of the temperature of the medium in which the frogs had lived, with the muscular current which they gave: I will describe once for all and in a few words, my mode of experimenting. From November 1844 to the end of March 1845, I have constantly sent twice a week to have frogs caught from the same marsh. As soon as the frogs arrived at Pisa, they underwent the usual mode of preparation for determining the intensity of their muscular current. A certain number of these same frogs were put into a glass recipient without water, and kept in a little room, the constant temperature of which was $+16°$ C. (60°·8 F.) A similar number of the frogs were placed in a like recipient, but exposed, on the terrace of the meteorological observatory, to the temperature of the atmosphere. Lastly, four frogs taken from the mass were put into the bottle furnished with a tube (fig. 3.), the tube A B of this bottle dipped in mercury. The apparatus thus disposed was likewise left exposed to the temperature of the atmosphere upon the terrace. In eight hours' time the tube C was opened, and inflating the bladder, a portion of the air was collected, and the quantity of carbonic acid produced by the four frogs in that time, and in those given conditions, determined. It is impossible for me to relate all the experiments performed for the space of five months, twice a week; comparing the frogs just arrived fresh from the marshes, with those kept for twelve and twenty-four hours at a temperature of $+16°$ C. I shall merely state some of the numbers given by these experiments, in order to render the results of so many experiments, all concordant, more evident. The temperature of the air, or of the medium in which they live, the activity of the respiratory function, the intensity and duration of the muscular current, are quantities which vary from one another proportionally; the temperature of the medium in which the frog lives cannot be raised without occasioning an increase of activity in its respiration, and a corresponding increase in the intensity and duration of the muscular current.

Four frogs exposed to a temperature varying from 0° to 4°, produced in twenty-four hours 0·5 cubic centimeters of carbonic acid; four other frogs placed in the same recipient and in the same conditions, but at +16° C, gave 0·3 cubic centimeters of carbonic acid. The celebrated experiments of EDWARDS give double this number at a temperature of +27° C. We now come to the signs of the muscular current. In the coldest days of the past winter twenty elements gave a deflection of 32°, then in succession, as the temperature of the air increased, 38°, 48°, 50°, 56°, 60°, 66°. These indications correspond to the gradual increase of the temperature from 0° to 8°. Finally, in the present month, the thermometer rising to +15° in the shade, the intensity of the muscular current was expressed by the numbers 80°, 85°, 90°. It is needless for me to repeat that these numbers refer to the first deviation which takes place on closing the circuit of the pile of twenty half thighs of frogs, always disposed in the same manner, and with their extremities immersed in the two cavities of the board, filled with spring water.

I attained to the same result, comparing the intensity of the muscular current produced by frogs recently caught in the cold season, with that obtained from frogs which had been left for the space of twenty-four hours to forty-eight hours exposed to a temperature of +16° C. The following are some of the many numbers noted down, for the sake of demonstrating, even in this case, the relation between the intensity of the muscular current and the temperature in which the frogs have lived. A pile composed of frogs caught in the coldest part of the season have produced a deflection of 32°. The deflection caused by other similar frogs which were kept for two days in a warmer temperature, was 38°. In another case the muscular current rose from 30° to 48°, in another from 50° to 64°, in another from 66° to 85°.

I would observe, however, that if the frogs are kept too long exposed to a warm temperature, and deprived of the medium in which they generally live and are nourished, instead of the increase of the muscular current, produced by an increase of temperature taking place, a considerable diminution of the current follows in comparison with that of the frogs recently caught. Experience has also shown me (that which was easy to foresee), that in proportion to the elevation of the temperature in the frogs originally, so much the sooner the effect of the want of nutrition was manifest.

I think it important to describe a few experiments which establish the relation between the intensity of the muscular current and the activity of the respiratory function. I have only been enabled to try these experiments upon frogs, from their great tenacity to life. I have repeated the following experiment several times: I skinned ten frogs completely, and put them into a glass recipient near to another in which were ten other frogs intact. The frogs thus flayed live for six, eight, and even ten hours, and are even quite lively. The muscular current produced by the skinned frogs was always considerably weaker than that of the frogs in their natural

state. In four experiments made upon the skinned frogs, left in this state from two to eight hours, the first deflections were from 80° to 85°, and the needle first remained stationary at 18°, then continued to fall slowly. In four other experiments made upon frogs in their natural state, the first deflection was 90°, and the needle was stationary at 25°, then fell as usual.

I have performed a few experiments upon the muscular current of warm-blooded animals. In one of these experiments, communicated by M. DUMAS to the Academy of Sciences at Paris, and in which I composed the muscular pile, with live pigeons, I succeeded in obtaining the signs of the muscular current. The result of that experiment was to show me that the intensity of this current increased in proportion to the rank the animal operated on occupies in the scale of animals, while the persistence of the current diminished in the same proportion. Operating with great rapidity upon chickens and pigeons, I have been able to demonstrate the truth of this, using for my experiments the thighs of the above-mentioned animals. Comparing an equal number of elements, whether of fowls or pigeons, with the same number of elements taken from frogs, the current, at first, is as intense, and in the greater number of cases more so than that of the frogs. Reflecting a moment on the greater length and resistance of the circuit of the pile of fowls and pigeons, the greater intensity of the muscular current in warm-blooded animals than in frogs, will be manifestly proved. This advantage, however, persists but for a very short space of time: a pile of eight elements of half thighs of pigeons or fowls, at the expiration of an hour, gives either no sign at all, or an almost imperceptible sign of a muscular current in the most delicate galvanometer I possess. This is far from being the case with the same number of half thighs of frogs, which continue for eight hours and more to manifest signs of the same current.

Nor is this difference owing to an unequal evaporation, whether from the internal or external surface of the muscle. I have very frequently bathed the surface of the muscle with pure water, and made a fresh surface, by cutting it away with a razor, and reconstructing the pile: I have never found more than a slight increase in the first deflection, and none whatever on waiting until the needle became fixed.

In order still better to confirm some of my former experiments for showing the influence of the sanguineous circulation upon the intensity and duration of the signs of the muscular current, I compared the current produced by twenty elements, or half thighs of frogs, in their natural state up to the moment of conducting the experiment, with twenty others, from which the heart had been taken away, but which still preserved considerable power of motion for a great length of time; by this means I have established the conclusions to which I had arrived, proving that the muscular current is very much weakened by the defect of the sanguineous circulation. But the following experiment, better than all those referred to formerly, will demonstrate the truth of this last conclusion. From a quantity of frogs all caught in the same

pool, I chose twenty, which I put into water that had boiled for two hours. I covered the glass cylinder in which this water was contained, with a plate of glass which I luted to the cylinder. To prevent the water from again taking up air, I covered the surface with oil. The temperature of the water was +15° C. The frogs appeared more vivacious at the commencement of the experiment, from their continual movement from the surface of the water to the bottom, and *vice versâ*, but it was not long before this vivacity ceased, and in about an hour they were all at the bottom of the cylinder, showing signs of suffering, and with but little motion. In two hours all motion entirely ceased and the frogs seemed dead. I have repeated this experiment twice with the same result, the only difference discoverable was in the time, which varies inversely as the temperature of the medium. The signs of the muscular current are considerably weakened in a pile composed from frogs which have been labouring under asphyxia for some time. Thus, while a pile composed of frogs which had not been submitted to any previous injury, caused a first deflection of 90°, and the needle remained fixed between 25° and 30°, with a pile equally composed of twenty elements, but which had been taken from frogs in a state of asphyxia, the first deflection was not more than from 50° to 60°, and the needle stopped at from 10° to 12°. This difference is very striking, and the influence of a normal state of the sanguineous circulation and respiration could not be more clearly demonstrated. I will mention here a singular appearance of the muscles in frogs asphyxiated: the muscles are almost white, and acquire a slightly red tint from exposure to the air. I was desirous to renew and verify the singular action of sulphuretted hydrogen. The following numbers, the result of various experiments, show that the muscular current is very much weakened in frogs killed by this gas. Twenty elements, or half thighs of frogs killed in the usual manner, gave a deflection of 56°. Another twenty elements taken from the same mass gave 44°, another similar pile 41°.

I will now sum up the results obtained from these different experiments. In the first place, the intensity and duration of the muscular current are independent of the nature of the gas which envelopes the muscular pile. Secondly, this current, as I have already shown from the commencement of my researches, is altogether independent of the cerebro-spinal nervous system, and the circumstances which exercise a marked influence upon its intensity are respiration and the sanguineous circulation. Thirdly, those poisons which seem to act directly upon the nervous system, have no influence upon the muscular current; among these I would mention hydrocyanic acid, morphine and strychnine. Fourthly, sulphuretted hydrogen has a marked influence in diminishing the intensity of the muscular current. Fifthly, the intensity of the muscular current varies according to the temperature in which the frogs have lived a certain time; it is needless to observe that this result is not discoverable except in those animals which like the frog necessarily take their temperature from that of the medium in which they live. Sixthly, the intensity of the muscular current

increases in proportion to the rank the animals occupy in the scale of beings, while the duration of this current after the death of the animal is in an exactly inverse ratio.

Comparing these conclusions with those generally admitted by physiologists, and drawn from a great number of experiments on the vital properties of muscles, it is impossible not to perceive that the property of the muscles, immediately connected with the muscular current, is that which HALLER calls irritability, and which at the present day I believe physiologists designate by the name of organic contractility, or simply contractility.

With regard to the manner of representing the origin of the muscular current, I only find in my present experiments a confirmation of the opinion I set forth in my preceding ones. The chemical action which goes on in the nutrition of the muscle, principally that which takes place in the contact of the arterial blood with the muscular fibre, is in all probability the source of this electricity in the muscles. I will not here repeat all the many and minute researches, together with the precautions I took, in the numberless experiments attempted in order to exclude every possible cause of a current foreign to the muscle; and nobody who has taken the pains to follow out the description of my experiments, can retain any doubt that the origin of the muscular current is in the muscle endued with a certain degree of vitality. The experiments referred to, making the pile act in vacuum, in hydrogen, in oxygen, and in carbonic acid, prove with full evidence that it is not the action of the gas upon the inner surface of the muscle which occasions the current. To remove all possible doubt, I have taken the precaution of preparing the half thighs of frogs with gilded scissors or with pieces of glass made sharp at the edges; the muscular current was the same, as in fact it ought to have been, as indeed it should have been.

It might be said, reasoning according to a theory the value of which is well-appreciated at the present day, that the cause of the muscular current resides in the contact between the inner and the outer part of the muscle, or in other words, of two heterogeneous bodies. Let this then be the simplest interpretation of all the facts discovered by myself; it is sufficient for me to have well-established that this contact of heterogeneous parts of the muscle generates electricity in the conditions discovered, and such as they are described in the results referred to above.

With regard to my own opinion, it appears more satisfactory to say that the development of electricity takes place in the muscle during life from the chemical action between the arterial blood and the muscular fibre; that the two electric states evolved in the muscle neutralize each other, at the same points from which they are evolved, in the natural conditions of the muscle; and that in the muscular pile imagined by myself, a portion of this electricity is put in circulation just as it would be in a pile composed of acid and alkali, separated from each other by a simply conducting body.

I will conclude this memoir upon the muscular current, with the description of an

experiment which appears to me to tend to establish the origin of the muscular current according to these views.

I prepared a great number of little cones, of about the size of half a frog's thigh, cutting the very thin membrane of the cæcum intestine into triangular pieces, and folding them up, and gumming them together upon a little wooden form of a conical shape. When these cones were dry, I prepared some fibrine by beating up some bullock's blood, the bullock being killed at that moment. I immediately fitted the cones with this fibrine steeped in blood, and composed a pile of twenty elements precisely similar to the pile of half thighs. This pile did not exhibit the slightest signs of a current in the most sensible of my galvanometers. Nor is it to be imagined that there was any want of conductibility in the pile just described; in fact, I added four thighs of frogs prepared to the above twenty elements, and I obtained from this pile a deflection but slightly differing from that which the frogs gave, acting alone. This fact evidently proves that simple heterogeneity of the animal parts is not sufficient to produce the muscular current; this heterogeneity should be such as exists in the living muscle.

XII. *Electro-Physiological Researches.—Second Memoir. On the proper Current of the Frog.* *By Signor* CARLO MATTEUCCI, *Professor in the University of Pisa, &c. &c. Communicated by* MICHAEL FARADAY, *Esq., F.R.S., &c. &c.*

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IN the seventh chapter of my Traité des Phénomènes Electro-Physiologiques des Animaux, after having examined all that GALVANI, HUMBOLDT, VALLI, and more recently NOBILI, have said on this subject, I mentioned all my own researches from which the laws of this phenomenon follow. Comparing the muscular and the proper current together, we find that the influence of the different circumstances affects both currents equally. Thus it is that both the muscular and the proper current vary in the same sense with the variation of the temperature of the medium in which the frogs live. Sulphuretted hydrogen diminishes the proper current just as it does the muscular current. The same may be said of the effects produced upon the proper current by the different degrees of activity of the respiration and of the circulation of the blood. I shall not here mention all the numbers obtained in the experiments recently performed upon the proper current of the frog. I will merely say that I followed up every one of the experiments on the muscular current referred to in this memoir, with another experiment on the proper current, composing the pile with the legs which remained after having prepared the frogs for the muscular current. After so great a number of facts, I do not hesitate in repeating what I have said in page 127 of my Treatise: "comparing one with the other the circumstances which exert an influence over the muscular and over the proper current, they may be said entirely to resemble one another, and that that which increases or weakens the intensity of one of these currents, produces the same effect upon the other." Two points, however, still remain to be cleared up. Do the circumstances which affect these two currents operate upon both in a like degree? or in other words, does that circumstance which diminishes as well the muscular as the proper current of the frog, act proportionally in that diminution?

I had found in my first experiments, that comparing two piles, one consisting of half thighs of frogs, the other of entire or halves of frogs, so as to obtain the proper current, the signs produced by the latter pile continued longer than those evinced by the muscular pile.

I then began to renew my former experiments for the purpose of convincing myself of the reality of this difference, the only one between the muscular and the proper current.

Studying afresh the influence of the various circumstances (temperature, respira-

tion, sanguineous circulation, sulphuretted hydrogen) upon the proper current compared with the muscular current, I came to the following conclusion: the diminution which occurs in the intensity and duration of the proper current, from the decrease of temperature, from defect of respiration and the sanguineous circulation, and from the action of sulphuretted hydrogen, is considerably greater than that which takes place in the intensity and duration of the muscular current. Thus, with the same number of elements, I have always seen that the proper current has been considerably weaker than the muscular current, operating on frogs asphyxiated, killed with sulphuretted hydrogen, or in the coldest weather. This difference decreases in proportion to the robustness and vivacity of the frog, so that in the spring and summer, choosing very strong frogs, the signs of intensity and the duration of the proper current equal and even surpass those of the muscular current.

I was desirous of again studying the proper current in piles composed either of legs alone or of half thighs of frogs, or of entire frogs (figs. 5. 7. 8). In general, as I found before in my former experiments, these three piles produced currents the intensity of which was sensibly alike in all. This coincidence seems at first rather singular, considering the diversity of the internal resistance of the three piles, and admitting that the electro-motor element of the proper current resides in the leg alone. The experiments which I shall report at the close of this memoir, explain this fact sufficiently clearly.

Comparing, however, the three above-mentioned piles, composed from frogs which had been exposed previously to the action of debilitating causes, I have always found that the signs of the proper current, in the pile composed of the legs alone, somewhat exceeded those of the other piles.

I would here refer to another experiment, described at page 116 of my Treatise, performed with a pile (fig. 6.) of half frogs, from which the upper half of the thigh had been taken away. In this pile the proper current is in opposition to the muscular current, for which reason the current obtained is very weak, and sometimes null. I have still, however, constantly observed that if the frogs employed for this experiment are very robust, and in those conditions which we have seen to be favourable to the proper current, the signs of a current which this pile gives, though always very weak, are in favour of the proper current; while, on the contrary, when the frogs are taken in the conditions unfavourable to the proper current, the slight current which this pile gives are in favour of the muscular current.

From the sum of these facts I am again forced to conclude, as I was led by my former experiments to do, that the proper and the muscular current are in general subjected to the same laws, and that both these currents vary in the same sense, under the same circumstances.

But why should the proper current belong exclusively to the frog? This is the problem the solution of which I had long been anxious to arrive at, and hope finally to have given a satisfactory explanation.

I had frequently observed, while operating with great rapidity upon rabbits, fowls, and pigeons, that the signs of the proper contractions frequently manifested themselves, and that therefore the celebrated experiment of GALVANI was repeated in warm-blooded animals. When the thighs are cut away from these animals, the nerve laid bare, and folded back upon the leg, contraction is frequently visible. These contractions are more constantly obtained by composing piles of these thighs and making the nerve touch the leg. I must say, however, that every time that I have composed a pile of such thighs, I have obtained the signs of the muscular current, for which reason the contractions might have been attributable to that current.

Let it be remembered, however, that in composing these piles, it is impossible not to put the current which parts from the internal surface of the muscle in circulation, for which reason a pile should be made analogous to that of the half frogs divided at the upper half of the thigh (fig. 6.). Observation has shown, from the time of GALVANI, that the points of the leg of the frog to be touched, for the purpose of producing the proper contraction, are the points of insertion of the surface of the funicular tendon of the gemellus or gastrocnemius muscle into the calcaneum.

In one experiment, described at page 105, I had endeavoured to remove the tendinous surface of the muscles of the legs, then composing the pile with the frogs so prepared, I had the current as at first, that is, directed from the feet to the head, in the animal. This experiment, however, did not prove that the proper current exists independently of the tendinous surface of the muscle; in fact, removing the tendon, I lay bare the muscle, and so doing prepare a muscular pile, in which the current, being directed from the interior to the exterior of the muscle, is therefore in the same direction as the proper current.

The following are the experiments which led me to generalize the fact of the proper current of the frog. There is no difficulty in preparing the gemellus or gastrocnemius muscle of the frog, leaving a certain portion of the funicular tendon, or tendo Achillis, which goes on to insert itself into the calcaneum, and taking care to avoid as far as is possible injuring the upper part of the muscle. I prepared a great number of these elements, and arranged them in a pile, as represented in Plate IV. fig. 9, in such a manner that the tendinous extremity came in contact with the belly of the muscle. From this pile I obtained signs of a current directed in the muscle from the tendon to the muscle, that is to say, in the same direction as the proper current. Comparing together an equal number of elements arranged in piles, and consisting of legs alone, or of gastrocnemius muscles only, the intensity of the current has been to all appearance the same.

With equal facility the rectus femoris of the frog may be prepared, leaving the tendinous extremity which is inserted into the patella, and laying bare as little as possible of the internal muscular surface of the upper part. Thus I have been enabled to form a pile composed of several recti femoris, always arranging them in such a manner that the tendinous extremity reposed upon the surface of the muscle as far

removed as possible from the internal part. A pile so formed has given constant and distinct signs of a current directed in the muscle from the tendon to the muscle. With regard to the intensity, I must add that the signs have always been weaker, from the pile composed of recti femoris, than those from the pile of half legs or gastrocnemii muscles.

It is very natural that the cause of this difference should be owing to the fact of the muscular current circulating in a contrary direction to the other. And, in truth, if the disposition of the elements which compose these piles be ever so little changed, so that the tendon of one of these elements be made to repose upon or near to the interior of the muscle, the signs of every current become very weak or cease altogether (fig. 10.).

I have prepared a number of anterior cubital muscles, or muscles of the fore-arm of frogs, which likewise have at their extremities, near the carpus, a very distinct tendinous band. A pile composed of these muscles, disposing them as usual, with the tendon resting on the muscular surface of the next element, gives constant and very distinct signs of a current, the direction of which, in the muscle, is from the tendon to the muscle. The following in the meanwhile is the generalization of the fact of the proper current of the frog: the current is directed within the muscle from the tendon to the superficies.

It remained for me to extend this fact to its operation upon the muscles of warm-blooded animals, and the experiments accorded in such a manner as to leave no possible doubt.

In these experiments I employed fowls, pigeons, rabbits and dogs. It is necessary to operate with great rapidity upon these animals, since, as in the muscular current, the signs of the current which we are now studying cease very quickly. Not less than six or eight elements are necessary for eliciting signs of this current sufficiently evident to remove all doubt. In all these animals the muscular extremities turned towards the feet are furnished with tendons much more distinct and grouped together than those of the upper and opposite extremities. I wished at first to have separated the different muscles as I had done those of the frogs, but the process is much more difficult with the muscles of these animals, which always get considerably lacerated.

In order to succeed in the best possible manner, after having removed the integuments, I cut the thigh as near as possible to the articulation with the os ilium; and in pigeons it is easy to tear the thigh out of the socket. The surface of these elements should be well-dried and the pile formed (fig. 11.), disposing them in such a manner that the inferior extremity of the leg, where the tendons unite together, reposes upon the surface of the muscular masses of the leg. In this manner the muscles of the thigh have no part in the circuit. From similar dispositions of eight elements taken from rabbits or pigeons, the signs of a current, marked by my galvanometer, were from 12° to 15°, and 20°, and directed in the pile from the tendinous extremities

to the muscular surfaces. It is sufficient to introduce the thighs into this pile, to put, that is to say, the interior of the muscle in contact with the tendinous extremity, for the sign of the current to be inverted, and the muscular current produced (fig. 12.). This proves how necessary it is, in order to have the signs of the current directed from the tendon to the muscle, not to comprehend any portion of the interior of the muscle in the circuit.

Let us then conclude, that "touching a mass of muscle belonging to a living animal, or an animal recently killed, with a homogeneous conducting arch, one extremity of which is contact with the tendon of the muscle, and the other with the superficies of the muscle itself, signs of an electric current are obtained, which circulates in the muscular mass, its direction being from the tendon to the external surface of the muscle."

This fact comprehends that of the proper current of the frog.

Let it not be forgotten that from the sum of all our researches, it has been proved that both the muscular and proper current are subject to the same laws, and thus in all probability have a common origin. I would here again call the attention of anatomists to the study of the structure of the muscles, and of the relation which exists between the muscular fibres, the tendon, and the membrane which invests the fibres or the sarcolemma.

If I have rightly understood the classical labours of my friend Mr. Bowman, it would follow that the extremities of the elementary muscular fibres are immediately connected and continued with the tendinous fibre; while the sarcolemma which invests the muscular fibre ceases abruptly where the tendon begins. On the strength of this disposition I cannot abstain from emitting an hypothesis upon the origin of the proper current, which would reduce all that we know on the subject of animal electricity to one principle alone. Let it be granted that the tendinous fibre, from its structure, from its connections with the muscular fibre, and from its conductibility, represents the internal part of the muscle, and that the sarcolemma, on the contrary, is distinguished under this aspect from the muscular fibre; then the case of the proper current, or of the current from the tendon to the muscular surface, becomes at once the simplest and most general case of the muscular current. We must never forget the analogy between the muscular electro-motor element and the Voltanian element: the zinc is represented by the discs of the muscular fibre, the acid liquid by the blood, the platinum by the sarcolemma. Whatever be the conducting body with which the zinc is made to communicate with the platinum, the current is always in the same direction. If it be well proved by anatomy that the tendinous extremities are continuous with the extremities of the muscular fibres, and that the sarcolemma which envelopes the muscular fibre alone, and not the tendon, is not continuous, is not as it were identified with the muscular fibre, the analogy between the muscular element and that of Volta is complete and perfect.

The chemical actions of nutrition evolve electricity.

Pisa, April 7, 1845.

XIII. *Electro-Physiological Researches.—Third Memoir. On Induced Contractions.*

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Communicated by W. BOWMAN, *Esq., F.R.S.*

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THE term *Induced* contractions was applied in England to a physiological fact discovered by myself, and described in the tenth chapter of my treatise on the Electro-physiological Phenomena of Animals. I shall henceforth adopt this denomination, since it has the advantage of expressing the phenomenon with brevity, and, to a certain degree, its nature.

I will begin by explaining, in a few words, in what this fact consists, together with the principal researches which I made in the commencement for the purpose of discovering its laws. Having prepared a galvanoscopic frog, I laid its nerve upon one or both the thighs of a frog prepared in the ordinary manner; this done, on applying the poles of a pile upon the lumbar plexuses of the frog, at the same time that the muscles of the thighs were contracted, contractions were excited in the galvanoscopic leg, the nerve of which reposed upon the thigh of the other frog. I discovered the same fact, placing the nerve of the galvanoscopic frog upon a muscle of the thigh of a rabbit, and exciting the muscle to contraction by means of a current which traverses its nerve. I have even seen contractions of the galvanoscopic frog occur without applying the electric current for the purpose of contracting the muscle which ought to induce the contractions, adopting for this purpose any other stimulus to the lumbar plexuses or to the spinal marrow. I finally tried these experiments, introducing between the nerve of the galvanoscopic frog and the inducing muscular surface very fine laminæ of different substances. A leaf of gold and a very fine non-conducting stratum of mica or of glazed paper being interposed prevented the phenomenon, that is to say, the induced contractions in the galvanoscopic frog failed to appear, whilst a stratum of fine paper soaked in water did not interrupt the induced contraction. From the whole of these facts I was led to conclude,—1st, that the contraction induced in the galvanoscopic frog could not be attributed to the effect of derived currents; 2nd, that it should rather be considered the effect of an electric discharge taking place during the contraction of a muscle. For the sake of supporting this explanation of the induced contractions by facts, I instituted a great number of experiments which are described in the tenth chapter above referred to. With this view I composed a pile of entire frogs, and closed the circuit with the two extremities of the galvanometer. Allowing the needle to become stationary, I touched specially the nerves of the frogs composing the pile with a solution of potassa, by which means

contractions were excited in these frogs. Operating in this manner, I have often remarked the deflection of the needle to be increased by a few degrees, after which the needle retrograded. When the frogs were touched several times with the potassa, or were very much weakened, so that touching them again with the alkali no longer produced contractions, it has, in most cases, occurred that there was no sign of increased deflection in the needle of the galvanometer. Finally, bathing the nerves of frogs arranged in piles with acid or saline solutions, the deflection, far from increasing, rapidly diminished, at least in the beginning.

These facts, with which I paused, might have appeared in some manner favourable to the idea that the induced contractions were the effect of an electric discharge which accompanies the act of muscular contraction: notwithstanding this I terminated the chapter referred to with the following words:—"I cannot take upon me to affirm that the question is entirely solved, and I pause here from not knowing how or by what way to advance to solve it."

The importance, however, of the fact of induced contractions thus always appeared to me very great, and consequently I have not failed to give my strictest attention to the study of it, and I have reason to believe, latterly, with some success. I shall, therefore minutely describe in this memoir all the experiments that I have instituted upon the induced contractions, and I trust the reader will excuse the prolixity of the description.

Before commencing a fresh series of investigations into the fundamental fact of the induced contractions, I thought it necessary to repeat and vary the experiments of which I have already given an outline, and which were directed to the purpose of discovering whether there is a development of electricity during the contraction of a muscle. In order to have a more fixed and considerable deflection, it was necessary to employ piles consisting of a greater number of elements than those I had made use of previously. I imagined that for this purpose a muscular pile would be far preferable to a pile of frogs. I avail myself of this opportunity of referring to an experiment made for the purpose of proving the existence of the muscular current in the living human subject. I applied the nerve of the galvanoscopic frog with care to the muscle of a leg laid bare by a wound. The most lively contractions were excited in the galvanoscopic frog every time that the circuit was suitably closed between the interior of the wound and the surface.

My late experiments have shown beyond all doubt that, the number of elements taken from the same frogs being equal, the muscular current is much stronger than the proper current. I have recently shown that when from defect of nutrition, a very low temperature, or the action of sulphuretted hydrogen, &c., both the muscular and the proper current of the frog are weakened, the diminution is much more considerable in the latter than in the former. And in fact, composing the pile described at p. 116 of my treatise with halves of frogs prepared by cutting the thighs in half, I find a differential current varying in intensity, but always in the direction of the muscular current. It is only in very robust frogs, dividing the thigh in the thickest

part, and leaving a very small internal surface of the muscle exposed, that the differential current is found to be either null, or in the direction of the proper current. This fact attracted my attention in the course of my early experiments, but the explanation of it is clearer to me since my later investigations, reflecting that in leaving the thigh almost entire, we have two elements, that is to say, the muscles of the leg, and those of the thigh, which direct the current in the same direction, while there is but one element of the muscular current which gives a current in a contrary direction.

Returning to the subject of this memoir, I would observe that I have employed a muscular pile in the view of determining whether there is evolution of electricity in the contraction of a muscle. But since in order to excite the contractions of the muscle, I was obliged to bathe it with concentrated saline or acid solutions, or better with alkaline solutions, I previously studied the effects of the action of these liquids upon the muscular current. In this view, from a great number of frogs I selected eight which I prepared in the usual manner, and which furnished sixteen elements or half thighs. Closing the circuit, the needle reached 90° and remained stationary at 22°. Constructing another similar pile, after having well-washed and then dried the sixteen half thighs, the result was the same. Another sixteen similar elements were immersed for a few seconds in a diluted solution of sulphuric acid, and afterwards washed several times in water, so that they did not redden turnsol paper. Composing the pile and closing the circuit, a current was produced in the direction of the muscular current, but the deflection was only 6° or 7° at first, and the needle was fixed at 0°. I rapidly cut the half thighs with a pair of scissors so as to renew the internal surface of the muscle in a fresh state. Recomposing the pile, the current was still weaker than that indicated above. Thinking that the effect of the acid solution upon the muscular elements was to diminish the conductibility, I constructed a muscular pile of eight half thighs of frogs taken from frogs previously intact, to which I added four entire thighs taken from frogs likewise intact: the current which resulted was 46°. In the room of the four entire thighs, I next substituted four entire thighs which had been immersed in sulphuric acid and then washed, the current was 44°. The conducting power therefore had not varied in the muscular masses subjected to the action of the acid solution. To be still more certain of this conclusion, I tried the experiment already described, substituting eight half thighs for the four entire ones for the purpose of prolonging the circuit. These eight half thighs being treated with the acid solution, and joined together by contact of the internal surface of one with the external surface of the other, the result was the same.

I next repeated the same experiments, using a sufficiently concentrated solution of potassa in which to immerse, for a few seconds, the muscular elements or half thighs. These elements were then washed in pure water until they showed no alkaline reaction. Composing the pile with sixteen elements, and closing the circuit, there was a first deflection of from 10° to 12° in the usual direction of the muscular current,

and an imperceptible stationary deviation. Making a fresh section of the muscle and recomposing the pile, the result was the same. And in this case, likewise, the conductibility was not changed; consequently the alkaline or acid solutions act as I had before found water act at a high temperature. I will here repeat an experiment which I made, merely to show its accordance with those related above. Sixteen half thighs of frogs were immersed for a few seconds in water at about 50° C. On taking these elements out of the warm water and bathing them with cold water, I constructed a pile, closed the circuit, and had a first deflection of 12° in the direction of the muscular current, and the needle stopped at 0°. After having renewed the internal surface of the muscle by making a fresh section, I recomposed the pile, and the signs of the current were the same as before. And also in this case I assured myself of the fact, that the conductibility had not been sensibly changed by the action of the hot water. I will add, moreover, that it is not to the repeated washing in pure water at the ordinary temperature, that the diminution in the intensity of the muscular current is to be attributed. I have very frequently seen the same deflection slightly varying in intensity, produced by a pile of a given number of elements, or half thighs, sometimes washed in pure water, at others not washed at all. Even a solution of chloride of sodium highly concentrated, is capable of diminishing considerably the signs of the current produced by a pile of which the elements have been immersed for some seconds. Thus, whilst sixteen common elements produce a first deflection which mounts to 90°, and remains fixed at from 20° to 22°, if these elements be left for a few seconds in the saturated solution of the sal marinus and then taken out, the first deflection is about 60°, and the needle becomes stationary at between 8° and 10°.

We are thus led to conclude, that by the action of the alkaline acid or saline solutions in a concentrated state, those conditions of the muscular elements by which the evolution of electricity takes place, are destroyed. Nor is this conclusion in opposition to the admitted origin of this current: but since by the action of the acid, or saline solutions, the signs of the muscular current either cease or are greatly weakened, it remains to be explained why, in the experiments reported in my work, and of which I have given an outline in the commencement of this memoir, the diminution of the current did not take place on touching the elements of a pile of frogs with an alkaline solution, while it occurred immediately on touching them with an acid solution.

On the contrary, we have observed, that operating with alkalies, at the first contractions that are excited, there is in many cases a perceptible increase of deflection, which lasts for some seconds. With acids, on the contrary, the deviation is lessened immediately, and returns again after a short time.

Before endeavouring to account for these phenomena, I will describe those of my experiments performed with the greatest exactitude, in the view of ascertaining whether there be in contraction any evolution of electricity. I prepared a great number of frogs according to GALVANI's method; I then cut their legs, disarticulating them

as well as I was able: there remained a couple of thighs united by a portion of the spinal chord. One of these thighs I cut in half, and in this manner I had a certain number of elements all alike and consisting of an entire thigh, a portion of the spinal chord, and a half thigh. It is easy to understand how, with these elements, I composed a muscular pile, that is to say, applying the external surface of the entire thigh upon the internal surface of the thigh of the succeeding element (fig. 13.). This done, I immerse the extremities of the galvanometer in the liquid in which the extremities of this pile terminate. A small appendage to the extremities of the conducting wires of the galvanometer, precludes the necessity of my holding the latter with my hands when I require to complete the circle. I have repeated this experiment a great number of times, at one time using a pile of twelve, at another of sixteen, at other times of twenty elements. Both the first deflection, and that at which the needle remained stationary, were somewhat weaker than they would have been had the piles been composed of an equal number of halves of thighs only. This difference is mainly attributable to the greater length or resistance of the circuit. In every case referred to above, after having suffered the needle to become stationary, which it did in the various experiments at 10°, 12°, and sometimes 15°, I touched the lumbar plexuses of the different elements with a sufficiently concentrated solution of potassa, excepting, however, the two extreme elements, for fear the alkaline solution should reach the liquid in which the extremities of the conducting wire of the galvanometer were immersed. The muscular contractions took place immediately upon the application of the alkali, and lasted for some seconds without ever being sufficiently strong to interrupt the pile by displacing the elements. During these contractions the needle of the galvanometer did not move. In some cases I have seen the needle go back, in others rise to 2° or 3°. But these variations are too uncertain in the greater number of cases, and most generally correspond to a too sudden motion of the elements of the pile which disturbs their contact.

Let us then conclude that direct experiment answers negatively to the question we proposed to solve, whether there were evolution of electricity in muscular contraction.

Having got rid of this part of the question, the next which presents itself is the consideration of the phenomena which we observe in the proper current (for which we employ whole frogs), and which consist in signs of increased intensity on first touching the lumbar plexuses of the frogs with potassa, while, on the contrary, an acid solution causes the needle to fall instantaneously. To arrive at a knowledge of these facts I have repeated and varied my former experiments, and the explanation of the facts is as follows:—

Whatever be the form of the elements used for constructing the muscular pile, that is to say, whether it be made of entire frogs, of halves of thighs, or, as described (fig. 13.), if the surface of the muscular elements be bathed with an acid or alkaline solution, it constantly happens, whether there be contractions or not, that the de-

flection diminishes and the needle returns to zero, where it remains if the action of the alkali be repeated, or the solution too concentrated. This effect is identical with that already described, which occurs in the muscular elements immersed for a few seconds in acid or alkaline solutions. In the manner of conducting the experiment (fig. 13.) which we have adopted, in order to excite the muscles to contraction, we touch with the alkali such points of the muscles as are in a certain manner out of the circuit, and which certainly do not constitute the electromotor part of the element. In the pile of entire frogs, with which we succeed oftenest in obtaining signs of increase of the current for a few seconds, on touching the lumbar plexuses alone with the alkali, these signs of increased intensity never occur if the entire muscular surface be bathed with an alkaline solution. I will add, moreover, that if an acid solution be employed, and care be taken to touch the lumbar plexuses only with a paint-brush, carefully avoiding the muscles of the thighs or legs, the deflection is not diminished, and in spite of the contractions which are excited, although these are less than those which the alkali occasions applied upon the muscles, there is no increased deflection. The surface of the muscle must be touched with the acid to make the needle fall. The same occurs with the alkali, and is, I repeat, in accordance with the experiments already referred to with the muscular elements which have been immersed in the acid or alkaline solutions.

It is then only with the pile of entire frogs, and only when the lumbar plexuses of these alone are touched with the alkali, that sometimes a slight increase of deflection is perceived, while this does not occur when acids are similarly applied. Taking up our ground upon the strength of all the experiments described above, it is impossible to consider this result as contrary to the absolute answer in the negative given above to the question whether there were development of electricity in muscular contraction.

In the course of the present memoir other irrefragable proofs will be adduced in support of the assertion to the contrary. It is impossible for anybody who looks well to the whole of these phenomena, not to perceive the difficulty which occurs in endeavouring to explain why, in the particular case described above, the alkali should produce an increase of deflection in the proper current of the pile of entire frogs. For myself, I incline to the belief that stronger and more permanent contractions being excited by the alkali than by acids, the contact between one element and the other becomes thereby more intimately established in the greater number of cases, and, by that means, the internal conducting power is increased. In effect, this contact is very imperfectly established in the pile of entire frogs, and very great difference in the intensity of the current is perceptible with the same elements by improving their mutual contact.

Whatever may be the right interpretation of the slight increase which occurs in the intensity of the proper current, on touching the lumbar plexuses of the frogs, and thus exciting muscular contractions, certainly this fact alone is insufficient to

lead us to admit the evolution of electricity during muscular contraction, while, as certainly, all the other cases described oblige us to conclude the contrary.

I will now proceed to detail several other new researches upon the phenomenon of induced contractions: but I must first entreat that the reader will excuse my trespassing on his time with a lengthened account of my numerous experiments: the fact of induced contractions is certainly of such importance, and at the same time so obscure, that it cannot be established except by long and patient researches.

No one who has once seen induced contractions occasioned by contractions excited by other means than an electric current, can hesitate to admit that this current is not the direct cause which produces them.

If the nerve of the galvanoscopic frog be applied upon the muscles of the thigh of a frog prepared in the ordinary manner, and the spinal chord be suddenly lacerated with scissors, or with the broken edge of a piece of glass, or by any other means, induced contractions very rarely fail to occur. It is however certain that passing an electric current through the lumbar plexuses, the phenomenon of induced contractions scarcely ever fails.

As I have most frequently resorted to the passage of the electric current for exciting contractions, I have taken every possible precaution to prevent any portion of the electric current from invading the galvanoscopic frog or the thighs of the entire frog. The best mode of conducting the experiment, and the way which offers the best chance of success, is to fill a common dinner-plate with Venice turpentine and spread the frog upon it. It is hardly necessary to say that the turpentine should be so dense that the frog cannot sink in it; care must be had in preparing the galvanoscopic frog not to leave any portion of muscle attached to the nerve.

Whatever may be the position of the nerve of the galvanoscopic frog relatively to the muscular fibres of the thigh on which it is laid, the phenomenon of the induced contractions always continues. Thus on some occasions I have extended this nerve parallel to the muscular fibres, or I have extended it normally to the said fibres, or in fine, I have bent it in a zigzag, that is, in all directions, and the induced contractions have been obtained in every case and without sensible difference.

These induced contractions are obtained by applying the nerve of the galvanoscopic frog on the gastrocnemius muscle of the leg.

I have also attempted, by washing the galvanoscopic frog several times in pure water, to remove any trace of blood or other humours which might be sprinkled over the surface of its muscles, and the induced contractions have equally continued.

I have cut with a razor, or better still with a pair of scissors, the surface of the muscles; I have then placed the nerve of the galvanoscopic frog upon the cut surface only of the muscles themselves; the induced contraction has taken place. It has also occurred on disposing the nerve of the galvanoscopic frog upon the muscle so that the extremity of the nerve should fold back over the nerve itself, and thus form a kind of closed circuit.

I have also wished to ascertain if the induced contractions continued even when the nerve of the galvanoscopic frog had not been cut. I have therefore prepared the frog, so as to preserve the nerve entire, as follows. Having skinned a frog I remove the viscera, then the bones and muscles of the pelvis, and finally the muscles of the thigh, taking care to preserve the nerve of the thigh. In this manner I obtain a frog whose nervous system is entire, and which has a long nervous filament uncovered, that is, the lumbar plexus and the nerve of the thigh. Having thus obtained the frog, I prepare another in the ordinary manner, which I place upon the turpentine in the manner already described. Then I place the nerve of the frog, prepared as has been said, upon the thighs of the other frog (fig. 14.). On exciting the muscular contractions, the induced contractions are obtained as they are by using the galvanoscopic frog alone; and at the same time the contractions in the muscles of the back and in the other leg are obtained. We shall have occasion to recur to this experiment further on; we now limit ourselves to deduce from it that the induced contractions are obtained, even when the nerve placed upon the muscles in contraction is entire.

In using the frog thus prepared, I have experimented upon the induced contractions, by causing the nerve that is in contact with the muscle in contraction to be already in some manner excited by a current or some stimulus. For this purpose I have comprehended the galvanoscopic frog in the circuit of a voltaic circle, or have applied upon the nerve a drop of an alkaline solution. Every time that the inducing muscles are contracted, there is always induced contraction, whether the nerve through which this current is transmitted be already excited or not, and consequently, even when the muscles in which the induced contractions are generated, are already in contraction; and, in fact, in spite of the contraction already present there is no difficulty in perceiving the induced contraction that follows.

Many easy experiments may be made to prove that in whatever way the nerve of the inducing muscle be excited, if its contraction fail, the induced contraction is also wanting. I shall limit myself to reporting some of the principal ones. Having cut the nerves at two or three points in the interior of the inducing muscle, so as to prevent its contracting, the induced contraction is wanting when the nerve is in any way stimulated external to the inducing muscle.

If, without cutting the nerve, all the tendinous extremities of the muscles of the thigh are severed, and transverse cuts are also made in those muscles, taking care not to divide the nerves, on stimulating them, the inducing and also the induced contractions are wanting.

By removing with care all the muscles of the leg of a frog, the nervous filament that runs in the leg itself may be uncovered. This nerve may be irritated either with the current or any other stimulus, after having extended the nerve of the galvanoscopic frog upon the muscles of the thigh above. These muscles of the thigh do not contract; the induced contraction is wanting.

By operating upon rabbits or upon dogs, I have been able, with the electric current, to act upon the nervous filaments that run to the kidneys, to the stomach, and to the intestines: when the nerve of the galvanoscopic frog was extended upon the different parts in the same conditions as for the muscles, I never obtained any sign of induced contraction.

I have also sought to discover if there was contraction induced by applying the nerve of the galvanoscopic frog upon the excited nerve. For this purpose it is sufficient to prepare two galvanoscopic frogs, and to extend the nerve of the one upon the nerve of the other, in the points nearest to the leg. To perform the experiment with every care, the two frogs are disposed upon turpentine. Then, either with the current or with some other stimulant, the superior points of the nerve of the frog, that I shall continue to call inducing, are erected. There is no induced contraction in the galvanoscopic frog, although this contraction immediately occurs, if its nerve is extended over the gastrocnemius of the other. It is needless to say that in using the current to excite the inducing contraction we must never place either of the electrodes of the pile in contact, or in proximity to the nerve of the galvanoscopic frog.

It is proved by the above experiment that an excited nerve, and one in which is certainly propagated that cause, whatsoever it be, which awakens contraction in the muscle and sensation in the brain, does not act upon the nerve of the galvanoscopic frog placed in contact with it. I will also add the following experiment. I have uncovered the brain of a frog prepared in the ordinary method with the greatest possible care, and I have extended upon it the nerve of the galvanoscopic frog.

In various experiments thus tried, I have applied the current sometimes direct, sometimes inverse, upon the lumbar plexuses, and in others I have touched these plexuses with potassa, and I always obtain the contractions in the lower limbs and convulsions in the back. However, I have never found signs of induced contractions in the galvanoscopic frog that was extended upon the brain. The induced contractions are therefore originated solely by the muscle in contraction.

I have sought to discover how these induced contractions grow weak, by causing them to be originated by means of a muscle whose own contraction was induced. In a word, I have examined the induced contractions of the second and third order, &c. For this purpose I prepare various galvanoscopic frogs, and one in the ordinary manner, and I dispose them in the following way. Upon the muscles of the thighs of the entire frog I extend the nerve of a galvanoscopic frog: upon the gastrocnemius of this, I extend the nerve of another galvanoscopic frog, and so on in succession. The whole is placed upon turpentine. On exciting the contractions of the entire frog by making the current pass through its lumbar plexuses, I have seen in many instances *three* galvanoscopic frogs contract, and all with nearly the same vivacity. The contractions are never wanting in two frogs, but I have never been able to perceive four contracted. There is therefore an induced contraction of the first, the second, and the third rank. Before coming to the consequences to be drawn from the above-

mentioned facts, it remains to me to describe the many experiments I have made for the purpose of discovering the influence of bodies interposed between the muscle in contraction and the nerve of the galvanoscopic frog upon the induced contraction. From my first experiments upon the induced contraction, I had perceived that on extending a sheet of gold leaf, such as is used for gilding, upon the muscle, and then placing the nerve of the galvanoscopic frog upon the gilded muscle, the induced contraction did not take place. That this should happen, it was necessary that the muscle should be completely coated with the gold leaf, which is not the case after one or two contractions when the gold leaf gets torn. I had then seen that a varnished paper (*papier glacé*) interposed between the muscle and the nerve impeded the induced contraction; and lastly, a sheet of felt soaked with water or the serous liquid that bathes the surface of muscles, and interposed between the muscle and the nerve of the galvanoscopic frog, does not prevent the induced contractions. Our knowledge was limited to these three cases relative to the action of interposed bodies upon the induced contraction. I have therefore sought to extend and vary the experiments. The manner of operating that I have adopted, consists in preparing a frog in GALVANI's method and placing it upon turpentine; while an assistant is preparing more galvanoscopic frogs whose nerves I extend upon the muscles of the thighs of the first frog. In order to awaken the inducing contraction, I always use a small FARADAY's pile of fifteen elements immersed in pure water, and whose electrodes are covered with silk and varnished.

There is no liquid body among the many examined that impedes the induced contraction; pure water, slightly acidulated and saline water, serum, blood, olive oil, diluted alcohol, the varnish of alcohol and resin, volatile oil of turpentine are the liquids made use of in these experiments, and through which the induced contraction takes place. I am always accustomed to let some drops of the liquid under experiment fall upon the muscle, and to dip the nerve of the galvanoscopic frog in the same liquid. The induced contraction still subsists even if a thin sheet of felt imbued with the above-mentioned liquids is interposed between the muscle and the nerve.

The slight conductibility of some of the liquids made use of (oil, oil of turpentine, varnish, &c.) has made me doubt whether the induced contraction would not subsist even in spite of the interposition of an absolutely insulating body. I assured myself, in fact, that across a layer, even very thin, of the said liquids, neither the muscular current nor the proper one was propagated. On holding the galvanoscopic frog in the hand, and causing its nerve to come into contact with a wetted paper that in any manner communicates with the ground, the contraction, as is well-known, is obtained. The same thing happens on touching with the nerve of the galvanoscopic frog, the muscles either of a frog or of any other animal in communication with the earth. In all these cases it is always the proper current that circulates through the observer, the ground, the touched body, and the galvanoscopic frog. Now if we wet the nerve of the galvanoscopic frog either in common oil, or in oil of turpentine, or in varnish,

the thin stratum that adheres to the muscle is sufficient to impede the circulation of the proper current.

It is therefore indubitable that if an induced contraction is propagated through a stratum of the bad conductors mentioned, this induced contraction cannot possibly be owing to a current generated in the contracting muscle, and passing thence into the nerve of the galvanoscopic frog.

Nevertheless these experiments were so important for the theory of the phenomenon of induced contraction, that I desired to try the effect of interposing between the contracting muscle, and the nerve of the galvanoscopic frog, a still worse conducting body than those mentioned. The body that has served me in these experiments has been Venice turpentine nearly solid, and rendered more or less liquid by adding to it a little volatile oil of turpentine. Having smeared over the thigh of a frog with this mixture, and wetted the nerve of a galvanoscopic frog with it, I prepare the experiment as usual, and the induced contraction continues. To prove the bad conducting powers of the mixture made use of, I hasten to say, that if I apply one pole of the pile with which I excite the contractions upon the stratum of the insulating mixture, of course without penetrating to the muscle, and I touch with the other pole the nerve of the galvanoscopic frog, the contractions are not excited in it. It is therefore proved from this experiment that the induced contraction propagates itself through a stratum of an insulating substance that prevents the propagation not only of the muscular and proper currents, but also of that current which excites the inducing contraction.

If the insulating stratum exceeds certain limits of thickness, and the mixture has not a convenient degree of fluidity, the induced contraction is wanting. It is however impossible to determine within what limits of thickness in the stratum and fluidity in the mixture this occurs; it is sufficient for me to have established by experiment that in some cases the induced contractions are obtained, while there is interposed between the nerve and the muscle an insulating stratum which certainly arrests the muscular and proper current, no less than an ordinary voltaic current.

I shall say, finally, that I have never succeeded in obtaining the induced contraction when using a solid body interposed, however thin it might have been chosen, and whatever might be its nature. For this purpose I used flakes of mica extremely thin, flakes of sulphate of lime, gold leaf, paper smeared with glue, and leaves of vegetables. The induced contraction is always wanting. It is however a very curious, and I believe even important fact in its consequences, to obtain the induced contraction through the skin of the muscles of the inducing frog. The experiment never fails of success, whether the inducing contraction be excited by the electric current, or by any stimulus applied to the lumbar plexuses of the inducing frog.

Having thus described a long series of facts relative to the circumstances that intervene in producing, in modifying, and in destroying the phenomenon of induced contraction, it might be believed that with the aid of these I might ascend to the physical theory of the phenomenon.

Unfortunately I am rather doubtful of it, and in this uncertainty I again beg the reader to follow me in the discussions that I shall be compelled to make upon the different hypotheses that may be imagined by which to interpret the phenomenon of induced contraction.

I. It is sufficient to have once seen the induced contraction originated by arousing the inducing contraction by any mechanical stimulus, to no longer have any kind of suspicion that the electric current used for exciting the contraction is propagated to the nerve of the galvanoscopic frog*. How must we understand the induced contraction of the second and third order? How are we to explain the fact that the induced contraction is wanting (although the current may be applied as usual upon the lumbar plexuses of the inducing frog), only because by the section of the nerves in the thigh the inducing contraction is prevented, or at least greatly weakened? Why is the induced contraction wanting when we apply the same current upon the nerves below the thigh in which there are no inducing contractions? Why, when we act with a current upon the lumbar plexuses of a frog already weakened so as no longer to have the contractions except at the beginning of the direct current or the cessation of the inverse, why in these cases alone is there induced contraction?

It is useless to continue to enumerate the objections that may be made to the interpretation of induced contraction by referring to the diffusion of the current exciting the inducing contractions, a diffusion that can in no way be physically conceived.

II. It might be suspected that the induced contraction was the result of a mechanical stimulus—of the shock of the inducing muscles, which, in their contraction, shake the nerve of the galvanoscopic frog. I have attempted many times (using extremely delicate galvanoscopic frogs) to produce motion in the muscular masses of the thighs in every possible way, and I never could see the galvanoscopic frog contract itself. If the occasion of the phenomenon were this shock, how could we explain the cessation of the induced contraction by the interposition of a very thin sheet of gold leaf or mica between the nerve and the muscle?

I have very many times tried to apply the nerve of the galvanoscopic frog upon plates of metal and glass, upon stretched membranes, upon cords of catgut while they were vibrating, and there was never any sign of contraction in the galvanoscopic frog. It is not then the shock of the muscle in contraction against the nerve of the galvanoscopic frog that occasions the induced contraction.

III. It happens very rarely that the contraction in the galvanoscopic frog is obtained when the nerve is being stretched over the thigh of the other frog, and this even when both are perfectly insulated. It is however certain that every time that this happens the occasion does not fail to be discovered. It either consists in

* Through excess of caution I have many times endeavoured to obtain the induced contraction by exciting the inducing contraction from the laceration of the spinal marrow by means of a fragment of glass; the induced contraction has taken place as if the inducing contraction had been excited by the current, or by any other stimulus.

the inside of the muscle being uncovered in some points, or because the nerve of the galvanoscopic frog remains united to some piece of muscle, that folding back again touches the nerve when this is extended over the thigh. It has also appeared to me that sometimes these contractions occur when the tendinous extremities and the surface of the muscle and of the thigh touch two points of the nerve of the galvanoscopic frog. So let us say that the induced contraction takes place constantly in all cases in which, by the care taken, the above-mentioned circumstances that may awake the contraction of the galvanoscopic frog are verified. We know too that having cut with scissors the muscular superficies of the thighs, and rendered them thus quite uniform, the induced contraction continues, when the nerve of the galvanoscopic frog is applied upon the new internal surface of the muscle. This induced contraction subsists even through the skin of the frog, and on interposing insulating liquid layers between the nerve and the muscle. We have seen that the insulating power of those layers was such as not to permit the circulation of the proper and muscular currents. How then can it be supposed that the induced contraction should take its origin from the above stated circumstances, even admitting that they may be rendered more active, or that they may be excited by the muscular contraction? These circumstances reduce themselves to the phenomenon of a muscular current or of a proper current, which ought to traverse the nerve of the galvanoscopic frog, while that nerve would be enveloped by a stratum of an insulating substance, which we have seen cannot be.

IV. The first idea conceived by which to interpret the induced contraction, was that of an evolution of electricity which might accompany the muscular contraction. There is evolution of heat in the act of contraction; and, according to the important observation of QUATREFAGE, which there is much necessity for repeating, in order to exactly establish its details, there would appear to be development of light in certain cases of muscular contraction; so that analogy might lead us to infer the probability of the production of electricity by the muscular current: besides, the few experiments that I made when I discovered the induced contraction might very well be interpreted on this hypothesis. An insulating body, as a lamina of mica or varnished paper, when interposed, impeded the induced contraction: and it could not be otherwise. The same thing occurred even when a lamina of gold perfectly discharging the electricity, that it is supposed is produced by the contraction, prevented the nerve from being traversed by it.

In spite of these first steps, which were flattering me into giving a very simple explanation of the induced contraction and were leading me at the same time to prove an important phenomenon in the muscular contraction, I am now constrained to entirely abandon this idea because it is contradicted by experiment.

In the beginning of this memoir, I referred at all possible length to the many experiments made by which to examine if there is any augmentation of the muscular current, or of the proper one in the act of contraction. All my efforts have been useless, and I have been obliged to conclude that experience does not prove that the signs of the muscular or proper currents increase in the act of muscular contraction.

We might believe it to be owing to an evolution of electricity independent of the muscular and proper current; but how can we suppose it so, when we see that the induced contraction propagates itself through certain insulating strata, as turpentine, oil, &c., while it does not take place if we use an extremely thin plate of mica? It might be supposed that the electricity developed in the muscular contraction could have acted by induction. In this hypothesis it is clear why the turpentine does not stop the induced contraction, but it still remains doubly obscure why with the extremely thin plate of mica this should take place. I have made an experiment by covering a galvanoscopic frog, placed upon a plate of glass, with a sheet of mica; the electric discharge of a jar passes between the knobs of the universal discharger upon the sheet of mica, and the contractions in the galvanoscopic frog are aroused. I shall not occupy myself at this moment in analysing these facts; it is sufficient at present to prove that there must be induced contractions through the plate of mica if the occasion of the phenomenon were an electric discharge. I will add finally, that I have very many times attempted, and always uselessly, to awaken the contractions in the frog, by holding the nerve of the galvanoscopic frog near and almost in contact with a metallic conductor traversed by the electric current.

To place myself in favourable circumstances that the induced current may be complete in the frog, I prepare it in such a manner that a long nervous filament (that is, one of the lumbar plexuses with its continuation in the thigh) may be uncovered. The frog is otherwise intact, and the two legs touch each other. I support the frog with silken threads, so that it may be horizontal, and that its nervous filament may be in contact and parallel with the voltaic conductor which is varnished. When every care is taken to insulate the frog, signs of contractions are never seen in it either at the closing or the opening of the circle of the pile. It is seen that by this disposition the induced circuit may take place in the frog. I have used a BUNSEN'S pile of ten elements without any result.

There is therefore given no experimental proof of that explanation of the phenomenon of induced contraction which admits an evolution of electricity in the act of muscular contraction.

We are still ignorant of the cause of muscular contraction, and we know nothing of this phenomenon except that it occurs on acting even at a great distance from the muscle upon the nerve that ramifies within it; that this action requires for its propagation the integrity of the nervous filament from the point at which it is acted upon to the muscle; that this propagation acts with a velocity which we cannot judge to be less than that of light and heat and electricity in their different media; that that which modifies, augments, or destroys the complication of the physico-chemical phenomena comprehended in the nutrition of the muscles, operates equally upon its contractility under any stimulus affecting the nerves; finally, that the phenomenon of the contraction of a muscle ought to be understood, with the condition that whatever be the cause of this phenomenon, it acts in accordance with the physical law of

elastic bodies. The phenomenon of induced contraction would seem to be a first fact of induction of that force which circulates in the nerves and which arouses muscular contraction.

Admitting that we cannot give a satisfactory explanation of the phenomenon of induced contraction by recurring to electricity or any other known causes, as I think I have abundantly proved, it appears to me that we cannot, confining ourselves to a first fact, as is that of induced contraction, interpret it differently from what we have done. The induced contraction is only a new phenomenon of nervous force, a phenomenon of which we have given the principal laws in this memoir. It seems to me therefore more just to call that henceforth *muscular induction*, which I have hitherto called induced contraction. I shall conclude this memoir with some applications of muscular induction to physiology.

By the experiment described above (fig. 14.), it is proved that the muscular induction is propagated in a nerve at the same time towards the two extremities—towards the muscle as well as towards the nervous centre. If the muscular induction not only acts upon the nerve in contact with the muscle but also through some interposed bodies, it is natural to admit that when a muscular mass enters into contraction by the irritation acting on one of its nerves, the phenomenon of induction should occur in all the other nerves. And even wishing to yield for a moment to the analogies that exist between the electric current and the nervous force, we might be led to believe that this induction should take place upon the excited nerve which is the cause of the contraction. Could we not perhaps from this deduce a physical explanation of a well-established physiological fact, that within certain limits the activity of the muscles increases in proportion as the contraction is aroused in them?

It also appears to me that a great number of those movements which occur in us and in animals independently of the will, but yet following others occasioned by the will, may be considered as phenomena of muscular induction. I leave to physiologists the continuation of these studies, which appear to me worthy of all their interest.

I shall terminate by citing an experiment which I think proves an action of this nature. Having prepared a frog in the ordinary manner, I cut one of the nerves that constitute one of the lumbar plexuses, and I divide it precisely at the point of exit from the vertebral column. Having extended the frog upon turpentine, I draw on one side the severed nerve, and I irritate it either with the current or with an alkali. Thus very strong contractions are produced in the thigh, and at the same time, if the frog is very lively, there are contortions in the back and movements in its superior limbs.

XIV. *On the Temperature of Man.*

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IT has been too generally taken for granted that the temperature of man in health, as measured by a thermometer placed under the tongue, is a constant one. I have endeavoured to prove from the results of observations, that this is not strictly correct; that when not disturbed by disease it is subject to variation, to rise and fall under certain influences, especially of heat and cold, rest and exercise*.

In the present communication I propose to submit to the Royal Society some further observations on the same subject, made with an instrument better adapted for the inquiry than the medical thermometer commonly used, and which has afforded results of a precise and satisfactory kind.

The thermometer I have employed is a bent one, about twelve inches and a half long, its bulb about an inch long, and, where widest, half an inch thick; its curvature about three and a half inches from the bulb, and its stem, to which the scale is attached, nearly at right angles to the bulb, so that when inserted under the tongue, the observer has no difficulty in distinguishing accurately the degrees himself, whether near-sighted or the contrary; in the latter instance using merely a common magnifying glass. Each degree of the scale is a little more than half an inch (·6 inch), and is divided into ten parts; and each of these parts is sufficiently large to admit of subdivision by the eye.

It may be right to premise a few words regarding the manner of observing with this instrument; and to notice some precautions which it is necessary to take to avoid error.

First, as to the placing of the thermometer: it is requisite that the bulb should be introduced under the tongue, and as far back as possible; and that whilst in the mouth, respiration should be carried on entirely through the nostrils. If the thermometer is placed in the side of the mouth, between the teeth and the cheek, the temperature indicated is from three-tenths to one-tenth of a degree less, according to the degree of coldness of the atmosphere.

Next, as to time: it is necessary that the thermometer remain in the mouth many minutes, till the observer is sure that the maximum height is attained. If the mouth has been kept closed for a quarter of an hour previously, a shorter time is required, than if allowed to be open and the passage of respiration. This is well shown by trials with the thermometer raised a few degrees above the temperature of the mouth

* Researches, Physiological and Anatomical, vol. i. p. 162; and Philosophical Transactions for 1844, p. 61.

before introduction. In the one case, the thermometer slowly falls to the temperature of the mouth, and is stationary; in the other case, after having fallen it again rises, continuing to rise till the maximum temperature of the closed mouth is acquired.

The observations which I have made with this thermometer have been altogether on myself; it would have been difficult indeed to have made them on another, with the requisite degree of accuracy, as they are tedious, demanding so much time and care. They were begun in August last, and have been continued almost daily up to the present time, with the exception of the greater part of the month of October, when they were interrupted until a second thermometer could be procured to supply the place of the first, which was then broken, and which was even more delicate than the second. It was my intention to have extended them to a period of twelve months before collecting the results; but this I am not able to accomplish, having received an order to prepare and hold myself in readiness for foreign service. Abroad I hope to be able to continue them, and as that will be in a tropical climate, I am the more desirous of communicating now the information I have already obtained; the comparison of the two sets may prove interesting.

In conjunction with the temperature under the tongue, I have in most instances noticed the pulse and respiration, considering it a desideratum so to do, and with the hope that the observations on the latter may be useful data, and may in some measure tend to throw light on the former, there being such an intimate connexion between them. The posture in which the pulse and respiration have been counted has always been a sitting one.

Of the many problems which might be proposed regarding the temperature of the body, I shall now only touch on a small number; and I shall be well-pleased if the information I have to give shall be considered merely as a contribution towards their solution, a beginning of an inquiry to be extended.

1. *Of the Variation of Temperature during the twenty-four hours.*

To endeavour to determine what is the extent of this variation, I have made on several occasions, observations every second or third hour, from the time of rising to that of going to rest, confining myself to the house during the whole time and to rooms of nearly the same temperature, the greater part of it, and varying but little my occupation. The following for a single day will give a pretty accurate idea of the result:—

	Temperature under tongue.	Pulse.	Respirations.	Temperature of air of room.	
April 13. 7 A.M.	98°·5	54	15	49°	Just after rising.
9 A.M.	98·4	74	16	53	Just after breakfast.
11 A.M.	98·4	60	15	53	
2 P.M.	98·7	54	15	55	
4 P.M.	98·9	54	15	55	
5 P.M.	98·7	54	15	55	
6½ P.M.	98·3	62	16	57	Shortly after dinner.
7½ P.M.	97·7	66	15	56	Before drinking tea.
11 P.M.	98·1	52	15	64	
14. 1 A.M.	97·6	54	15	60	

During the whole period, I have almost constantly tried the temperature under the tongue on rising and before going to rest, and in many instances in the middle of the day, between 2 and 4 P.M., when the circumstances were favourable. These observations I shall give in detail in tables appended; here it may be sufficient to notice the mean of each month's observations.

	Temperature under tongue.			Pulse.			Respirations.			Temperature of air of room.		
	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.
August	98°7	98°5	98°0	56·9	52·5	53·1	15·3	15·1	15	61°	63°	67°
September ..	98·8	98·9	98·0	59·3	55·2	55·8	15·5	15	15·4	66	63	65
November ..	98·9	98·6	97·9	57·8	57·6	54·5	16·7	17·1	15·8	51	48	64
December ..	98·7	98·2	97·9	58·9	55·2	56·5	15·7	16	15·4	42	47	60
January	98·8	98·07	97·9	58·7	59·3	57·9	15·5	15·3	15·1	45	55	60
February ..	98·6	98·6	97·9	55·5	54·4	52·3	15·5	15	15·1	42	48	61
March	98·74	98·59	97·93	57	53	54	15·1	15·1	15	46	54	60
April	98·66	98·57	97·88	56·5	54·8	53·6	15·4	15	14·8	54·6	59·8	62·4
	98·74	98·52	97·92	57·6	55·2	54·7	15·6	15·4	15·2	50·9	54·7	62

During this period, comprising eight months, the health of the observer (aged fifty-five) was pretty good, almost uninterruptedly so, excepting in December and January, when he experienced slight lumbago, not preventing the taking of exercise; and for a few days in November and January an attack of catarrh in a mild form.

As I wish to be as concise as possible, I shall comment very little on the results of the summary of observations. They seem to prove in a decided manner that the temperature under the tongue, when under no disturbing influence, is about its maximum after waking after the repose of the night; that it continues high, but fluctuating more or less (probably owing to disturbing circumstances) till towards nightfall; and that it is lowest about midnight. Its lowness at the last-mentioned time is the more remarkable, as the temperature of the room in which the observer sat at night was almost uniformly higher than of that which he occupied during the day.

2. *Of the Variation of Temperature during different seasons of the year.*

The following Table exhibits the mean results of the observations made during the eight months, at the different periods of the day, both of the temperature under the tongue and of the air of the room.

	Mean temperature under the tongue.	Air of room.
August	98°4	63°7
September ..	98·57	64·7
November ..	98·47	54·3
December ..	98·27	49·7
January	98·36	53·3
February ..	98·37	50·3
March	98·42	53·3
April	98·37	55·5

These results give an average temperature of 98·4, that of the air being about 55·5. They show a slight relation between the temperature of the body and of the air, but less perhaps than might be expected, and less unquestionably than would have been exhibited under circumstances not equally favourable for the preservation of an equable warmth, especially at night, in the uniform temperature of the sitting-room; and when at rest, from warm bed-clothes, and during the day from sitting in cold weather near a fire, and from the clothing then, as well as at night, being varied with the degree of cold to be resisted, having in view the preserving of an agreeable feeling. The effect and perhaps best sign of the happy temperate mean in some facts which I shall have presently to bring forward, may aid in illustrating the remark just made.

3. *Of the Effect of Active Exercise on the Temperature.*

By active exercise, I mean that which occasions acceleration of the heart's action, and of respiration, and commonly a feeling of increased warmth, such as fast walking and riding, in contradistinction to the passive kind, as that which is taken in an easy carriage.

The following detail exhibits the results of observations made immediately after active exercise, in different months and under various circumstances:—

	Tongue.	Pulse.	Respirations.	Air.
August 15, 5 P.M. After fly-fishing by the river side, and riding about seven hours; feet and hands warm	99·4	80	18	63
August 17, 3 P.M. After a walk of three miles; gently perspiring	99·0	70	16	56
August 20, 5 P.M. After fishing five hours; gently perspiring	99·3	80	20	62
August 27, 2 P.M. After a ride (pretty fast) of five miles; feet and hands warm	98·7	58	16	64
August 29, 5 P.M. After a ride (pretty fast) of about fourteen miles; sun powerful; perspiring	99·5	84	18	64
August 31, 12 M. After a ride of ten miles; perspiring	99·1	60	16	65
August 31, 4 P.M. After an hour's walk; sun powerful; perspiring	99·3	64	18	70
September 2, 12 M. After a walk of two hours; the sun powerful; perspiring	99·2	64	18	72
October 30, 5 P.M. After a ride of ten miles (pretty fast); feet and hands warm	99·3	78	16	49
November 16, 4 P.M. After fishing two hours; slightly perspiring	98·9	62	18	55
December 31, 3 P.M. After riding and walking several hours; feet and hands warm	99·1	74	16	34
February 3. After a walk of seventeen miles; moderately warm	99·1	98	22	32
March 7, 5 P.M. After a mountain excursion on foot for several hours, and riding ten miles	99·2	90	17	33
March 20, 3 P.M. After a ride of ten miles; feet and hands warm	98·9	56	16	37
March 31, 1 P.M. After two hours' fishing; pleasantly warm	98·9	62	16	54
April 2, 1 P.M. After riding ten miles; moderately warm	98·9	62	16	54
April 11, 4 P.M. After five hours' fishing; not heated	99·0	70	16	40
April 17, 2 P.M. After four hours' fishing; slightly perspiring	99·2	84	18	55

These observations, selected from a large number of similar bearing, show in a decided manner, that active exercise, not carried to the extent of exhausting fatigue, raises the temperature of the body; and that the increase is, at least within a certain limit, proportional to the degree of muscular exertion made.

4. *Of the Effect of Carriage Exercise on the Temperature.*

The observations which follow, were made immediately after getting out of the carriage, which was a close one, and its windows commonly closed; and the dress worn, at the time of being out, was warm.

		Tongue.	Pulse.	Respiration.	Air.
Nov. 17. 1 P.M.	After a drive of 8 miles.	97°·7	52	18	53°
19. 3 P.M.	After a drive of 10 miles.	97·7	48	16	48
25. 2 P.M.	After a drive of 10 miles.	97·0	56	16	44
27. 12 M.	After a drive of 8 miles.	97·5	56	18	42
30. 12 M.	After a drive of 8 miles.	97·4	56	16	44
Jan. 5. 5 P.M.	After a drive of 7 miles.	97·7	50	17	32

Feet and hands cool, almost cold, as was experienced in all the preceding instances.

These results are strongly contrasted with those given in the preceding section, showing the exalting effect of active exercise on the temperature. I have other results, equally proving how gentle exercise, in a cold atmosphere, has a depressing effect, whether taken in a carriage, on horseback, or on foot, walking slowly.

5. *Of the Effect of Exposure to Cold Air without exercise.*

The few observations I have collected on this point, have been made the instant after returning from an adjoining church, the temperature of which in the cold weather of winter is little above the freezing-point, no attempt being made to warm it, and the congregation which assembles in it at that season being small.

	Tongue.	Pulse.	Respirations.	Air.
Nov. 24. 1 P.M.	97°·0	52	16	42°
Jan. 12. 1 P.M.	97·1	50	15	40
Feb. 9. 1 P.M.	96·7	48	15	33
March 16. 1 P.M.	95·9	44	16	32

In each of the above instances, in spite of warm clothing, the sensation experienced by the observer was that of disagreeable chilliness, and in the feet and hands, of coldness; a feeling of drowsiness was also perceived, as if the condition induced were an approach to the state of temperature of a hybernating animal, or to that which is probably the prelude to the sleep in the human being resulting from long exposure to severe cold without exercise.

6. *Of the Effect of Excited and Sustained Attention on the Temperature.*

The state of mind referred to is that accompanied with exertion, such as is experienced in composition, or in reading a work of exciting interest.

The observations which follow have been made entirely at night, after from two to five hours of sustained attention. Many more were made by day; but these are not given, as they are not so well fitted for comparison.

	Tongue.	Pulse.	Respirations.	Air.
	°			°
Aug. 19. 12 P.M.	98·45	58	15	68
29. 11 P.M.	98·5	62	16	62
Sept. 23. 1 A.M.	98·5	54	16	65
Nov. 26. 12 P.M.	98·4	56	15	60
28. 12 P.M.	98·7	60	16	62
Dec. 14. 1 A.M.	98·5	56	15	64
20. 1 A.M.	98·7	58	16	60
30. 1 A.M.	98·0	56	16	55
Jan. 23. 12 P.M.	98·35	60	14	61
Feb. 3. 1 A.M.	98·4	60	17	60
12. 12 P.M.	98·2	58	16	60
21. 1 A.M.	98·4	54	15	60
24. 2 A.M.	98·4	58	14	60
26. 2 A.M.	98·0	56	15	60
Mar. 4. 1 A.M.	98·5	56	15	60
11. 12 P.M.	98·5	52	14	60
14. 1 A.M.	98·2	54	15	61
April 3. 12 P.M.	98·4	58	16	68
	98·4			

These observations show an increase of temperature after sustained exertion of mind. Though the increase is slight, yet I think it must be admitted to be decided, comparing the mean (98·4) with the average result of the observations (97·92) made at the same period of the twenty-four hours, when the attention was not roused, when it was rather in a passive indolent state, as in reading merely for amusement, or in the mechanical process of copying writing, both which seem to have, as is indeed generally believed, rather a sedative influence than an exciting one; and are to the former very like what passive bodily exercise is to active muscular exertion.

7. *Of the Effect of taking Food on the Temperature.*

The following observations were made after rising from the dinner-table, at which the observer commonly sat down at 5 o'clock, and partook pretty fully, using a mixed diet,—never taking anything between the breakfast and dinner-hour,—and using wine commonly at the latter meal, to the extent of three or four glasses, to the exclusion of malt liquor.

	Tongue.	Pulse.	Respirations.	Air.
	°			°
Aug. 15. 7 P.M.	98·2			
22. 6½ P.M.	97·9	60	16	60
25. 6½ P.M.	98·1	62	15	59
27. 6½ P.M.	98·4	58	16	62
28. 7 P.M.	98·6	76	16	68
29. 7 P.M.	98·3	82	16	63
Sept. 2. 6 P.M.	98·5	68	18	17
3. 6 P.M.	98·3	60	15	70
8. 8 P.M.	97·8	60	15	65
22. 6½ P.M.	98·4	70	15	55
29. 6 P.M.	98·5	68	16	62
Nov. 16. 7 P.M.	97·9	62	15	60
23. 7½ P.M.	98·1	70	18	54
Dec. 21. 7 P.M.	97·9	70	14	63
28. 7 P.M.	97·7	64	15	58
29. 8 P.M.	98·0	70	15	55
Jan. 2. 6½ P.M.	97·9	68	15	55
Mar. 24. 6½ P.M.	98·5	66	15	52
	98·1			

The majority of these results (the mean temperature of the whole being 98·1) seem to prove, that the amount of heat is reduced by a full meal. In the observer's case, as in most others, drowsiness followed this meal, thus approximating the condition of the animal system to that which precedes sleep. On particular occasions, when a larger quantity of wine than usual was taken, the reduction of temperature was commonly most strongly marked. A light meal, such as that of breakfast, consisting of tea, with a portion of toasted bread with butter, and often an egg, has had little effect in depressing or altering materially the temperature. It may be noticed, as regards the habits of the observer, in connexion with the observations on temperature made at a late hour, that after dinner he never took solid food, only two or three cups of tea, and this about 8 P.M.

The preceding observations, generally considered, appear to indicate clearly that the temperature of man, as determined in the manner described, is like the animal functions and secretions, constantly fluctuating within certain limits; and like them, observing in its fluctuation a certain order, constituting as it were two series; one regular, as the diurnal, connected with rest and refreshment from rest; the other, casual or accidental, depending on varying circumstances of irregular occurrence, as exercise, mental exertion, exposure to heat, and the contrary.

As the observations brought forward have been made on one individual, the inferences from them as regards extended application, can be held to be only probable, but probable, I cannot but think, in a high degree, the average temperature of the observer being nowise peculiar; and the results moreover being what might be expected reasoning on the subject, taking for data the proportions of oxygen which have been ascertained to be consumed, and of carbonic acid evolved in respiration, at different periods of the twenty-four hours, and under different circumstances.

Should observations similarly made on others present the like results (and I cannot but be confident that they will), more particular inferences may be drawn from them, especially in conjunction with respiration and the heart's action, not without interest to physiology; and they may admit of important practical application to the regulation of clothing, the taking of exercise, the warming of dwelling-rooms, in brief, to various measures conducive to comfort, the prevention of disease, and its cure. A step in advance is made, if it is only determined, as I believe it to be, that in the healthiest condition of the system, there is danger attending either extreme, either of low uniform temperature, or of a high uniform temperature, and that the circumstances which are proper to regulate variability within certain limits, not prevent it, are those which conduce most to health, as well as to agreeable sensation, enjoyment and length of life.

The Oaks, Ambleside,
May 1, 1845.

Appendix.

The Tables which follow, containing the monthly observations, require little additional explanation. It may be right to state, that they do not include the observations made under the influence of accidental disturbing circumstances, as active exercise, &c., the most distinct of which have been given apart in a section appropriated to them. The observations in these Tables, made under ordinary circumstances, or nearly such, will, I believe, be useful for comparison with the former, and I would hope, for reference, in progress of inquiry. In most instances, it will be found on comparison, that an unusual elevation of temperature has been followed by unwonted depression, and *vice versâ*.

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.
	°	°	°							°	°	°
Aug. 6.	98·3	....	97·9									
7.	98·8	....	98·2	56	....	54	13	....	15	63	....	68
8.	98·6	98·5	97·8	56	52	52	13	14	16	63	60	66
9.	98·8	....	98·2	60	....	62	14	....	16	63	....	70
10.	98·8	....	97·8	....	....	70	....	....	14	....	....	70
11.	98·5	98·6	98·0	56	50	58	14	15	16	62	59	67
12.	98·6	....	98·0	....	....	58	....	....	14	....	....	68
13.	98·7	98·5	97·7	52	52	58	14	15	16	61	62	68
14.	98·8	98·7	98·1	56	60	52	15	16	15	62	64	61
15.	98·7	....	....	58	....	....	16	....	....	64		
16.	98·5	98·5	97·6	56	50	50	16	14	15	63	63	63
17.	98·8	98·5	98·1	54	50	50	15	15	14	62	62	68
18.	98·7	98·4	98·2	62	54	56	16	15	16	60	68	68
19.	98·8	98·4	98·4	66	50	58	18	14	15	60	63	68
20.	98·7	....	97·9	54	....	58	16	....	15	62	....	68
21.	98·6	....	98·0	58	....	56	16	....	15	62	....	66
22.	99·0	....	98·0	56	....	58	16	....	15	60	....	68
23.	98·6	....	97·7	52	....	52	16	....	16	60	....	68
24.	98·5	98·5	97·8	52	50	48	16	16	14	60	61	68
25.	98·7	98·8	98·4	56	54	60	15	15	16	60	54	66
26.	99·0	....	98·1	58	....	50	15	....	15	60	....	65
27.	98·7	....	98·2	54	....	52	15	....	15	60	....	62
28.	98·7	98·8	98·1	60	54	54	15	17	17	62	72	68
29.	98·8	....	98·5	54	....	62	16	....	16	61	....	62
30.	98·9	98·8	97·7	62	54	50	16	15	16	60	63	63
31.	98·7	....	97·8	62	....	58	16	....	16	65	....	68
	98·7	98·5	98·0	56·9	52·5	53·1	15·3	15·1	15	61	63	67

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.
Sept. 1.	98°·7	98°·7	97°·8	62	56	56	17	16	15	67°	67°	67°
2.	98·8	98·7	98·1	54	54	54	16	16	16	68	71	68
3.	98·9	98·7	98·3	58	54	54	16	15	16	70	71	68
4.	99·0	....	98·1	60	....	48	16	....	14	72	....	70
5.	98·8	98·6	98·1	62	54	60	16	16	15	70	68	68
6.	98·8	....	....	60	....	....	15	...	....	69	....	....
7.	98·7	98·7	98·2	60	60	56	15	16	16	68	68	68
8.	98·8	98·9	97·8	54	60	52	15	15	16	62	65	64
9.	99·0	99·0	98·1	58	.. .	50	15	...	15	64	...	64
10.	99·0	....	....	60	....	....	15	....	....	64	....	.. .
11.	....	....	97·6	....	....	50	....	....	....	....	....	67
12.	98·7	....	98·2	56	....	50	15	....	15	62	....	64
13.	98·8	98·8	98·1	66	52	54	16	17	16	62	60	68
14.	99·0	98·7	98·0	62	52	50	16	16	15	61	60	60
15.	99·0	99·0	98·2	66	58	54	16	18	17	63	60	69
16.	98·8	....	97·9	66	....	52	16	....	15	62	....	68
17.	98·6	....	97·7	64	....	64	15	....	16	62	....	68
18.	99·0	....	98·4	62	....	50	17	....	14	62	....	61
19.	99·0	99·0	97·9	64	60	54	15	16	16	60	62	63
20.	99·0	....	98·3	58	....	52	15	....	16	60	....	58
21.	99·0	....	98·1	60	....	54	16	....	15	58	....	58
22.	99·0	99·0	98·5	54	48	54	15	16	16	58	55	65
23.	99·0	....	97·7	56	....	52	16	....	14	65	....	68
24.	98·7	....	98·3	54	....	56	14	....	15	58	....	63
25.	99·2	....	97·2	58	....	48	16	....	16	63	....	66
26.	98·3	99·0	98·3	58	56	54	16	16	16	58	58	67
27.	98·9	....	98·0	58	....	50	16	....	15	60	....	68
28.	98·6	....	97·7	60	....	62	16	....	14	62	....	69
29.	98·6	98·9	98·3	54	56	52	15	16	16	59	63	68
30.	99·0	....	....	56	....	....	16	....	....	58	....	
	98·8	98·9	98·0	59·3	55·2	55·8	15·5	16	15·4	66	63	65

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.
Nov. 1.	98°·75	98°·8	97°·6	54	58	....	16	17	16	53°	54°	62°
2.	98·8	98·5	97·7	60	58	54	17	18	16	52	44	60
3.	98·6	98·5	97·9	52	48	48	16	17	17	50	55	65
4.	98·7	....	97·9	60	....	70	16	....	16	51	....	66
5.	98·9	....	97·6	64	....	52	16	....	16	52	....	68
6.	99·2	....	98·0	66	....	58	17	....	16	50	....	63
7.	98·7	98·7	98·3	58	58	58	16	16	16	51	46	62
8.	99·5	....	97·8	63	....	54	17	....	17	52	....	68
9.	98·8	....	....	62	....	....	16	....	....	51	....	....
11.	99·3	98·3	97·8	62	61	50	17	17	15	52	54	68
12.	98·6	98·7	98·2	60	56	52	17	17	15	47	40	65
13.	98·8	98·7	98·1	56	62	54	16	17	16	52	50	62
14.	98·5	....	98·0	58	....	58	16	....	16	51	....	64
15.	98·8	98·7	97·9	56	56	60	16	17	17	54	55	63
16.	98·9	98·9	97·7	56	62	58	15	18	15	55	55	68
17.	98·7	....	97·5	52	....	52	18	....	15	53	....	65
18.	98·6	....	....	54	....	....	15	....	....	55	....	....
19.	99·1	....	98·2	60	....	56	17	....	16	55	....	66
20.	99·1	....	98·1	56	....	56	17	....	15	55	....	65
21.	98·9	....	98·0	60	....	58	17	....	15	55	....	62
22.	99·0	....	98·0	60	....	56	16	....	14	55	....	66
23.	99·0	....	98·2	60	....	62	16	....	16	53	....	65
24.	99 0	....	98·2	58	....	62	16	....	16	52	....	64
25.	99·2	....	98·1	56	....	56	17	....	16	48	....	60
26.	98·8	....	98·4	54	....	56	15	....	15	44	....	60
27.	98·7	....	97·4	60	...	50	16	....	16	43	....	62
28.	98·6	....	....	54	....	....	17	....	....	48	....	....
29.	99·2	....	98·2	54	....	60	16	...	17	49	....	64
30.	99·0	....	97·4	54	....	56	17	....	16	46	....	62
	98·9	98·6	97·9	57·8	57·6	54·3	16·7	17·1	15·8	51	48	64

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.
Dec. 1.	98°·7	98°·6	98°·3	62	58	50	17	16	16	50°	54°	64°
2.	98·8	....	....	54	....	....	16	....	....	48	....	....
3.	....	....	97·0	....	....	56	....	....	15	....	....	62
4.	98·7	98·7	97·7	56	60	50	16	19	15	44	32	58
5.	98·7	....	98·2	58	....	58	16	....	15	40	....	62
6.	98·9	97·8	98·3	56	56	60	16	17	16	38	43	60
7.	98·6	98·0	98·2	56	54	60	15	16	14	36	47	62
8.	98·5	98·4	98·1	56	50	54	16	16	16	38	43	60
10.	98·4	98·2	97·9	56	54	52	16	16	16	40	44	62
11.	98·5	....	....	58	....	....	16	....	....	40	....	....
13.	98·6	....	98·5	58	....	56	17	....	15	38	....	64
14.	98·4	....	97·9	60	....	58	16	....	16	40	....	58
15.	99·0	98·6	98·3	63	54	56	15	15	16	42	48	62
16.	98·4	97·9	98·1	56	56	66	16	15	16	42	58	60
17.	99·0	97·7	98·0	60	50	58	15	17	14	45	50	64
18.	98·7	....	98·2	60	....	58	15	....	16	45	....	62
19.	98·8	....	....	58	....	....	15	....	....	45	....	....
20.	98·8	98·0	97·5	64	56	54	15	16	15	43	43	65
21.	98·6	....	98·2	58	....	50	15	....	15	43	....	58
22.	98·8	98·4	97·8	52	52	62	16	15	14	43	50	66
23.	98·9	....	97·9	60	....	54	15	....	16	43	....	62
24.	98·6	....	98·2	60	....	54	15	....	15	....	....	65
25.	99·2	....	....	60	....	....	15	....	....	43	....	....
26.	98·8	98·4	98·3	58	64	52	16	17	15	41	34	50
27.	98·5	....	97·7	58	....	66	16	....	16	43	....	....
28.	99·3	98·7	97·5	66	58	52	16	16	15	41	53	55
29.	98·2	98·6	98·0	50	52	56	16	15	16	45	58	55
30.	98·8	98·9	97·3	60	62	66	16	15	16	43	58	58
31.	98·5	....	98·0	58	....	....	16	....	....	43	....	55
	98·7	98·2	97·9	58·9	55·2	56·5	15·7	16	15·4	42	47	60

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.
Jan. 1.	98·5	....	97·5	58	....	58	16	....	15	40	°	°
2.	98·4	....	....	54	....	....	16	....	....	42		
3.	99·0	....	97·9	64	....	58	16	....	17	42	....	55
4.	98·8	....	....	66	....	....	17	....	16	48	....	....
5.	99·1	....	98·7	64	....	60	18	....	15	46	....	56
6.	99·7	....	98·1	70	....	66	18	....	....	48	....	66
7.	100·0	98·9	98·1	72	60	52	18	17	16	50	58	65
8.	99·1	....	98·1	62	....	56	16	....	15	49	....	68
9.	99·0	....	98·3	60	....	60	16	....	16	46	....	61
10.	99·1	98·6	98·3	60	58	54	16	15	16	46	53	60
11.	98·7	....	97·4	56	....	58	14	....	14	47	....	60
12.	98·7	....	98·0	54	....	60	14	....	16	47	....	64
13.	98·9	98·7	98·1	52	60	62	16	14	16	46	53	61
14.	99·2	....	97·9	56	....	60	14	....	15	46	....	65
15.	98·8	....	97·7	58	....	56	14	....	16	47	....	64
16.	98·7	....	97·7	56	....	60	16	....	16	47	....	62
17.	98·9	....	98·1	54	....	60	15	....	14	47	....	62
18.	98·9	....	98·2	60	....	58	15	....	14	47	....	55
19.	98·8	....	97·8	60	....	56	14	....	15	47	....	60
20.	98·8	....	98·0	56	....	54	15	....	14	44	....	60
21.	98·7	....	97·9	58	....	62	16	....	16	44	....	63
22.	9·88	....	97·9	60	....	50	16	....	14	47	....	63
23.	98·8	....	....	56	....	....	16	....	....	47	....	....
24.	98·9	....	97·3	58	....	54	15	....	15	49	....	68
25.	98·6	....	....	54	...	....	15	....	....	47	....	....
26.	98·6	....	98·1	58	....	62	15	....	16	48	....	66
27.	98·7	....	97·9	58	....	54	15	....	14	45	....	62
28.	99·0	....	97·6	62	....	58	15	....	15	43	....	62
29.	98·5	....	97·9	56	....	56	15	....	15	42	....	60
30.	98·5	....	98·2	56	....	58	14	....	15	41	....	60
31.	98·8	....	97·9	60	....	62	16	....	16	38	....	62
	98·8	98·07	97·9	58·7	59·3	57·9	15·5	15·3	15·1	45	55	60

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.
Feb. 1.	98°8	....	97°5	60	....	52	16	....	15	39°	....	62°
2.	98·5	98·7	....	60	52	....	16	14	....	38	48	
3.	98·7	....	....	60	....	....	16	....	....	41		
5.	98·6	....	....	64	....	....	18	....	....	44		
6.	98·8	....	97·9	56	....	54	15	....	15	43	....	60
7.	98·5	....	98·1	56	....	62	14	....	15	39	....	60
8.	98·9	....	98·0	52	....	54	16	....	16	40	....	60
9.	98·1	98·5	97·9	52	56	54	14	15	15	38	50	61
10.	98·7	98·9	97·7	54	58	52	16	15	15	40	48	60
11.	98·7	....	97·7	56	....	54	16	....	15	40	....	60
12.	98·7	....	98·2	50	....	58	14	....	16	40	....	60
13.	98·4	....	98·2	50	....	54	15	....	15	40	....	60
14.	98·2	98·4	97·8	50	54	54	17	15	17	41	48	63
15.	98·8	....	97·6	54	....	54	17	....	15	43	....	60
16.	98·6	98·7	98·1	54	52	52	15	....	16	43	48	62
17.	98·5	....	98·2	54	....	50	16	16	15	44	....	60
18.	99·0	....	98·2	58	....	56	16	....	15	45	....	60
19.	98·7	....	97·7	56	....	48	15	....	15	44	....	60
20.	98·7	....	....	52	....	....	15	....	....	45		
21.	98·8	....	98·1	52	....	58	14	....	15	45	....	60
22.	99·0	....	97·4	60	....	60	15	....	15	44	....	62
23.	98·6	....	98·3	58	....	52	15	....	15	43	....	62
24.	98·3	....	97·8	58	....	54	16	....	16	42	....	62
25.	99·0	....	97·9	56	....	50	15	....	15	43	....	62
26.	98·3	....	98·0	56	....	60	16	....	14	46	....	62
27.	99·0	....	97·8	58	....	52	16	....	14	47	....	62
28.	98·7	....	98·5	54	....	60	15	....	15	47	....	60
	98·6	98·6	97·9	55·5	54·4	52·3	15·5	15	15·1	42	48	61

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.
March 1.	99°·0	98°·6	97°·6	58	56	58	16	16	16	45°	52°	60°
2.	99·1	98·7	97·9	58	52	46	16	15	16	44	55	60
3.	98·5	....	98·5	52	....	56	15	....	15	47	....	60
4.	99·0	98·3	98·0	60	54	54	15	16	15	45	52	60
5.	98·6	....	98·0	58	....	66	17	....	15	43	....	60
6.	99·0	....	98·2	60	....	54	16	....	15	43	....	60
7.	99·0	....	98·0	60	....	64	14	....	15	44	....	62
8.	99·2	....	98·1	60	....	60	16	....	15	46	....	60
9.	99·1	98·7	98·0	60	48	54	15	15	15	48	54	58
10.	99·0	....	97·5	60	....	54	16	....	15	48	....	60
11.	98·8	....	97·9	56	....	48	15	....	14	47	....	58
12.	98·4	....	97·7	54	....	56	16	...	14	47	....	61
13.	98·8	....	98·2	60	....	54	15	....	15	43	....	61
14.	98·6	....	97·9	54	....	46	14	....	14	44	....	58
15.	98·2	....	97·5	54	....	60	14	....	14	42	....	62
16.	98·7	....	98·1	58	....	62	15	....	14	43	....	58
17.	98·6	....	98·0	60	....	52	15	....	15	40	....	58
18.	98·5	98·7	98·0	62	58	48	15	15	15	43	53	60
19.	98·5	....	98·4	56	....	54	14	....	15	43	....	58
20.	98·6	....	97·5	54	....	54	15	....	15	42	....	60
21.	98·8	98·6	98·1	54	50	50	14	14	14	44	53	60
22.	99·0	98·5	97·8	56	54	54	15	15	13	48	58	60
23.	99·0	....	97·8	58	....	52	15	....	14	50	....	61
24.	98·7	....	98·0	58	....	48	15	....	13	53	....	58
25.	98·4	....	98·0	54	....	48	14	....	14	52	....	60
26.	98·4	....	98·2	54	....	60	15	....	15	51	....	62
27.	98·8	....	97·8	60	....	52	15	....	16	52	....	64
28.	98·7	....	98·3	54	....	54	16	....	15	50	....	64
29.	98·7	....	97·3	56	....	54	14	....	14	50	....	65
30.	98·6	98·6	97·8	55	52	48	15	15	15	51	54	62
31.	98·6	....	97·5	54	....	54	15	....	15	51	....	63
	98·74	98·59	97·93	57	53	54	15·1	15·1	15	46	54	60

Date.	Temperature under the tongue.			Pulse.			Respirations.			Temperature of air.		
	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.	7–8 A.M.	3–4 P.M.	12 P.M.
April 1.	98°3	....	98°0	58	....	58	16	....	15	53°	....	62°
2.	98·7	98·8	97·7	58	58	52	15	16	15	53	62	64
3.	98·8	....	97·7	56	....	48	15	....	14	55	....	62
4.	98·8	....	97·8	54	....	60	15	....	14	58	....	64
5.	98·9	....	98·1	60	....	64	15	....	16	58	....	64
6.	99·3	98·8	97·9	62	60	50	15	15	15	55	61	62
7.	98·8	98·8	98·2	54	56	56	14	15	16	58	61	63
8.	98·5	....	97·9	56	....	56	15	....	15	58	....	61
9.	98·6	....	98·1	60	....	58	14	....	15	54	....	62
10.	98·5	98·3	98·0	62	52	62	14	14	15	52	52	63
11.	98·7	....	97·6	60	....	54	14	....	15	51	....	63
12.	98·4	....	97·9	56	....	50	15	....	15	49	....	59
13.	98·5	98·7	97·6	54	54	54	15	15	15	49	55	60
14.	98·7	....	98·0	56	....	54	15	....	14	52	....	60
15.	98·6	98·8	....	56	60	....	15	14	....	49	58	
16.	98·4	....	97·7	58	....	52	15	....	15	52	....	62
17.	98·8	....	97·8	56	....	50	15	....	14	56	....	58
18.	98·4	98·8	98·1	60	54	54	15	15	15	60	60	66
19.	98·7	98·6	97·6	60	52	50	15	16	15	58	60	62
20.	98·7	....	98·3	58	....	54	15	....	15	60	....	64
21.	98·7	....	97·3	58	....	56	16	....	13	60	....	62
22.	98·4	98·1	98·0	60	56	50	16	15	15	62	65	64
23.	98·5	....	97·7	56	....	54	15	....	15	62	....	62
24.	98·7	98·2	97·7	58	52	52	16	16	15	64	64	65
25.	98·6	98·4	97·9	64	52	52	16	15	16	65	63	62
26.	99·0	....	98·0	58	....	50	15	....	14	62	....	62
27.	98·8	98·8	97·9	54	52	50	15	14	15	60	57	63
28.	98·6	....	....	58	....	....	16	....	....	58		
29.	....	98·3	98·2	....	54	52	....	15	15	....	60	64
30.	98·9	98·7	98·2	58	56	48	14	15	14	58	58	65
	98·66	98·57	97·88	56·5	54·8	53·6	15·4	15	14·8	54·6	59·8	62·4

XV. *Contributions to the Chemistry of the Urine. On the Variations in the Alkaline and Earthy Phosphates in the Healthy State, and on the Alkalescence of the Urine from Fixed Alkalies.* *By* HENRY BENCE JONES, *M.A., Cantab., Fellow of the College of Physicians.* *Communicated by* S. HUNTER CHRISTIE, *Esq., Sec. R.S.*

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On the Variations of the Earthy and Alkaline Phosphates in a healthy state of Urine.

HAVING observed the occurrence of a great excess of earthy phosphates in the urine in some cases of disease, and having made frequent examinations as to the quantity present on successive days, I found so great a discrepancy, that it became necessary before any further progress could be made to ascertain the variations in the amount of earthy phosphates in the urine of a healthy man, and, if possible, to trace the causes which determined the presence of an excess or deficiency of these salts in the urine. At the same time it was thought desirable to note the variations which the alkaline phosphates presented in the same water, and to see if they were influenced by the same, or by different causes.

A healthy man taking food twice daily, with moderate exercise for three hours, was the subject of the following experiments. The method followed was to take the specific gravity of the urine, if ever it was not strongly acid adding a drop or two of hydrochloric acid. Then from a weighed quantity, usually about 1000 grains, to precipitate the earthy phosphates by means of pure ammonia, to filter, wash with ammoniacal water, and heat them to redness, adding at last a drop or two of nitric acid. Thus the earthy phosphates were determined.

The alkaline phosphates were estimated by taking usually about 500 grains of urine, adding an excess of chloride of calcium and then pure ammonia; by this means all phosphoric acid was precipitated as phosphate of lime; this was filtered, well-washed, and heated to redness with nitric acid; by deducting the previously determined earthy phosphates, the difference was considered as alkaline phosphate*.

Though neither the calculation nor the method were perfectly accurate, yet they answered well for the purposes of comparison; and in disease the short delay before a result was arrived at which might determine the diagnosis, and sometimes the treat-

* The formation of a small quantity of carbonate of ammonia and the precipitation of some sulphate of lime, which even long washing cannot entirely remove, make the result too high. The equivalent of lime being less than that of soda tends to reduce the error. It must be remembered that the phosphoric acid is precipitated in combination with three equivalents of lime.

ment, was a matter of considerable importance; a few hours being usually all that were required to tell in what excess or deficiency the phosphates might be present.

I. (1.) Breakfast on bread and meat with coffee at 9½ o'clock. Dinner at 6: meat, potatoes, and little bread.

	Earthy phosphates.	Spec. gr.	Alkaline phosphates.
Water passed 6 o'clock, evening	·32 per 1000 urine	1022·8	6·50 per 1000 urine.
10 o'clock, evening	·97	1027·3	5·45
6 o'clock, morning	·81	1017·4	4·01
(2.) Food as before. More exercise. Dinner at 7.			
7 o'clock, evening	·37	1027·2	7·26
11 o'clock, evening	1·22	1029·9	6·06
5 o'clock, morning	1·41	1025·5	3·64
(3.) Food as before. Still more exercise.			
7 o'clock, evening	·75	1028·0	8·10
12 at night	1·29	1025·5	6·67
(4.) Food as before. Exercise very great excess.			
7 o'clock, evening	·21	1028·2	8·22
1 at night	1·85	1034·3	5·94
(5.) Food as before. Exercise much less.			
7 o'clock, evening	·35	1029·3	7·75
1 at night	1·91	1033·2	4·72

Average mean of five days.	Long after food, and soon after exercise.	Spec. gr.	Soon after food with perfect rest.	Spec. gr.
Earthy phosphates	·40 per 1000 urine	1027·9	1·45 per 1000 urine	1030·0
Alkaline phosphates	7·56	1027·9	5·77	1030·0

From this five days' experiment, it appears that the earthy phosphates are greatly increased in the water secreted soon after food; the quantity varying after dinner from 1·91 per 1000 urine, specific gravity 1033·2, to ·97 per 1000 urine, specific gravity 1027·3; the mean of all the experiments being 1·45 per 1000 urine, specific gravity 1030·0.

The earthy phosphates are far less in the water secreted a long time after food; the quantity varying from ·21 per 1000 urine, specific gravity 1028·2, to ·75 per 1000 urine, specific gravity 1028·0; the mean of all the experiments being ·40 per 1000 urine, specific gravity 1027·9.

The alkaline phosphates are in excess in the water secreted a long time after food and soon after exercise; the quantity varying from 8·10 per 1000 urine, specific gravity 1028·0, to 6·50 per 1000 urine, specific gravity 1022·8; the mean of all the experiments being 7·56 per 1000 urine, specific gravity 1027·9.

The alkaline phosphates are far less in the water secreted soon after food; the quantity varying from 6·67 per 1000 urine, specific gravity 1025·5, to 4·72 per 1000 urine, specific gravity 1033·2; the mean of all the experiments being 5·77 per 1000 urine, specific gravity 1030·0.

(6.) A child twenty months old, fed on bread with some meat and milk, gave in the water passed during the day,—

Earthy phosphates.	Spec. gr.	Alkaline phosphates.
·32 per 1000 urine.	1012·2	4·00 per 1000 urine.

(7.) On the same food.

Earthy phosphates.	Spec. gr.	Alkaline phosphates.
·33 per 100 urine.	1017·4	4·60 per 1000 urine.

II. I next endeavoured to ascertain on what the variations depended, and first with regard to food. For three consecutive days bread alone was taken with water, tea and wine, at the same hours as in the previous experiments.

(8.) The first day brown bread only.

	Earthy phosphates.	Spec. gr.	Alkaline phosphates.
5½ o'clock, evening	·27 per 1000 urine	1025·7	7·89 per 1000 urine
11 at night.	1·37	1030·0	6·39

(9.) The second day no analysis was made. The third day white bread only.

	Earthy phosphates.	Spec. gr.	Alkaline phosphates.
6 o'clock, evening	·37 per 1000 urine	1024·7	8·19 per 1000 urine
11 o'clock, evening	1·86	1032·1	5·56

(10.) Meat only was taken for three days with water, wine, and tea. First day exercise very little.

	Earthy phosphates.	Spec. gr.	Alkaline phosphates.
6 o'clock, evening	·42 per 1000 urine	1024·3	4·04 per 1000 urine
11 o'clock, evening	1·11	1021·9	4·21

(11.) The second day no analysis was made. The third day exercise rather more.

	Earthy phosphates.	Spec. gr.	Alkaline phosphates.
6 o'clock, evening	·48 per 1000 urine	1024·7	5·06 per 1000 urine
11 o'clock, evening	·81	1024·8	4·31

(12.) Meat only for dinner with distilled water, and tea with distilled water.

	Earthy phosphates.	Spec. gr.
5½ o'clock, evening	·33 per 1000 urine	1025·7
9½ o'clock, evening	·67	1026·5

From the comparison of these numbers with the previously given average, it appears that the earthy phosphates are not materially influenced by a diet of meat or diet of bread; that they are in excess after either is taken; and that even when animal food and distilled water alone were taken there was after food a decided increase, though the quantity was considerably below the average.

That the alkaline phosphates were in excess when bread alone was taken for food; and when meat alone was taken there was a considerable falling off in the amount excreted.

The next point was the effect of exercise.

(13.) Nothing was taken from dinner on the previous day to dinner this day; both meals consisted of mixed diet of meat, bread, and potatoes. The exercise was moderate, between three and six o'clock.

	Earthy phosphates.	Spec. gr.	Alkaline phosphates.
12½ o'clock	·45 per 1000 urine	1025·1	2·95 per 1000 urine
3 o'clock	·48	1026·0	2·92
6 o'clock		1027·1	total phos. per 1000 urine 4·77
10½ at night	1·02	1027·6	3·99

(14.) Nothing was taken since dinner on the previous day until dinner at six; mixed diet with more bread. Exercise also greater between 3 and 5½ o'clock.

	Earthy phosphates.	Spec. gr.	Alkaline phosphates.
3 o'clock	·36 per 1000 urine	1025·4	5·69
6 o'clock		1027·1	total phos. per 1000 urine 7·78
10½ at night	1·37	1032·5	5·50

(15.) Nothing was taken since dinner on the previous day, which consisted of meat only, with distilled water and wine. Very strong exercise was taken between 3 and 5½, the pulse always above 100.

	Earthy phosphates.	Spec. gr.	Alkaline phosphates.
11½ o'clock	·52 per 1000 urine	1022·9	3·04
3 o'clock	·36	1026·9	4·36
5½ o'clock		1028·9	total phos. per 1000 urine 6·81

In these experiments, the water, which was secreted longest after food, was not in sufficient quantity to admit of the determination of the earthy as well as alkaline phosphates. In all the experiments which were previously made, the exercise was always most between 3 and 6 o'clock, and yet at this time the earthy phosphates were always present in smallest quantity; so that the quantity of earthy phosphates does not appear to be quickly influenced by exercise.

The total quantity of phosphates which was found in the water secreted longest after food, and during strong exercise, was about one-third more than the total quantity previously present. This considerable increase so long after food, leads to the conclusion that the amount of alkaline phosphates is influenced by exercise, though, as appears from the previous experiments, not to the same extent as by the kind of food which is taken.

III. I pass now to the influence of different medicinal substances on the amount of earthy phosphates excreted.

(16.) Breakfast as before, at 9 o'clock. 15 grains of chloride of calcium taken in about an ounce of distilled water at 3 o'clock.

	Earthy phosphates.	Spec. gr.
3 o'clock	·30 per 1000 urine	1024·6
5½ o'clock	·22	1016·4

(17.) Experiment repeated.

	Earthy phosphates.	Spec. gr.
3 o'clock	·67 per 1000 urine	1024·4
5½ o'clock	·52	1020·8

(18.) Breakfast, bread only. 22 grains of chloride of calcium taken at ¼ to 1 o'clock.

	Earthy phosphates.	Spec. gr.
¼ to 1 o'clock	1·18 per 1000 urine	1028·6
3 o'clock	1·08	1025·2

(19.) Breakfast, bread and meat. 35 grs. of chloride of calcium in 1½ ounce of water at ¼ to 1.

	Earthy phosphates.	Spec. gr.
¼ to 1 o'clock	1·23 per 1000 urine	1026·8
3 o'clock	1·26	1023·8
5½ o'clock	1·08	1022·3
10¼ at night	1·82	1030·1

(20.) Breakfast, bread and meat. No chloride of calcium taken. Dinner as before at 6, chiefly meat.

	Earthy phosphates.	Spec. gr.
3 o'clock	·60 per 1000 urine	1027·4
6 o'clock	·36	1027·0
11 at night	·97	1032·7

From these experiments 15 grs. of chloride of calcium in an ounce of water produced no, or very little, effect in two hours and a half; 22 grs. in rather more water produced an increase in two hours and a quarter; 30 grains produced a still more marked increase in the same time, and the effect continued to be perceptible for at least ten hours.

(21.) Breakfast as before, with rather more meat. 30 grains of dry sulphate of magnesia were taken at 1 o'clock in about an ounce and a half of distilled water.

	Earthy phosphates.	Spec. gr.
1 o'clock water	·82 per 1000 urine	1026·6
3 o'clock water	·27	1026·0
5¼ o'clock water	·36	1025·8

(22.) Breakfast at 9 as formerly. 40 grains of dry sulphate of magnesia taken in about two ounces and a half of water at a ¼ to 1 o'clock.

	Earthy phosphates.	Spec. gr.
¼ to 1 o'clock water	·88 per 1000 urine	1029·3
3 o'clock water	·74	1031·0
5½ o'clock water	·90	1029·3
9½ o'clock water	1·64	1032·3

(23.) A patient of Dr. SEYMOUR's in St. George's Hospital had taken senna with

about two drachms of sulphate of magnesia in the morning, which did not act on the bowels; at 12 beef-tea and bread.

	Earthy phosphates.	Spec. gr.
3 o'clock	2·99 per 1000 urine	1027·6

A second experiment with the same urine, at the same time, gave nearly the same result; the alkaline phosphates were only 1·45 per 1000 urine, specific gravity 1027·6.

(24.) Another patient of Dr. SEYMOUR's in St. George's Hospital, who had taken senna and salts in the morning, with beef-tea and arrow-root for dinner, gave

Earthy phosphates.	Spec. gr.
2·93 per 1000 urine	1026·2

The quantity of sulphuric acid present in this urine
=3·21 per 1000 urine, specific gravity 1026·2
The amount given by BECQUEREL is = ·95 1018·9
The quantity stated by BECQUEREL is however below the average.

From these experiments, 30 grains of dry sulphate of magnesia in an ounce and a half of water produced no, or very little, effect in two hours. In four hours and a quarter the effect was distinctly visible. 40 grains in two ounces and a half of water produced a visible effect in two hours and a quarter, and a still more marked effect in four hours and three quarters. The effect continued to be perceptible for $9\frac{3}{4}$ hours after the medicine was taken.

As compared with the previous experiments, though the quantity taken appears to have been more, the effects seem not to have been so strongly marked; but in fact less of the base was taken in the last than in the previous set of experiments, for

30 grains of chloride of calcium are equivalent to 15·1 grains of lime,
40 grains of sulphate of magnesia are equivalent to 13·6 grains of magnesia.

As sulphate of magnesia is one cause of increase in the amount of earthy phosphates precipitated by ammonia, and as this salt also interferes in analyses regarding the quantity of sulphuric acid which is thrown out of the system, any means of knowing when it has been given as a medicine may be valuable. Most frequently it is prescribed with infusion of senna, which communicates a greenish yellow colour to the urine. This colouring matter, whether it has passed through the system or not, I find has the property of becoming of a deep red on the addition of an excess of any alkali, though it is most bright with ammonia. The red colour disappears again on neutralizing the alkali by an acid. It has a strong affinity for phosphate of lime. If rhubarb is taken, a less bright colour is given by the same reagents. I have lately found that Sir E. HOME in some of his experiments on absorption used the reaction of potash on tincture of rhubarb because it is so remarkable*.

(25.) Breakfast as before. 45 grains of dry magnesia taken at 10 o'clock. It had no action at all on the bowels.

* Philosophical Transactions, 1808, p. 45. According to the researches of SCHLOSSBERGER, this reaction is caused by chrysophanic acid which exists in the rhubarb.—Annalen der Chemie, vol. l. p. 214.

	Earthy phosphates.	Spec. gr.
11¼ o'clock, water acid	·90 per 1000 urine	1026·7
¼ to 1 o'clock, water alkaline	·69	1027·8
3 o'clock, water alkaline	1·19	1029·6
5½ o'clock, water very acid	1·48	1028·0
10 o'clock, water very acid	2·69	1032·8

(26.) Breakfast as before. 30·8 grains of magnesia taken at ¼ to 1. It did not act on the bowels.

	Earthy phosphates.	Spec. gr.
¼ to 1 o'clock, water acid	1·79 per 1000 urine	1030·4
3 o'clock, water acid	1·19	1032·2
4 / 5½ o'clock, both acid	1·69	1032·3
9½ o'clock, water very acid	2·44	1034·5

(27.) Breakfast as before. No magnesia taken.

	Earthy phosphates.	Spec. gr.
3 o'clock	1·00 per 1000 urine	1030·3
6 o'clock	·59	1031·4
9½ o'clock	1·39	1031·6

Hence 45 grains of magnesia produced no increase in the phosphates in two hours and three quarters; in five hours there was a marked increase; in seven and a half hours there was a still further increase, which was very marked at the end of twelve hours, and from the large quantity of earthy phosphates present the following morning, perhaps the magnesia had not ceased to act in twenty-four hours.

30·8 grains of magnesia produced no evident increase in two hours and a quarter; in four hours and three quarters the increase was marked, and after eight hours and three quarters it was quite perceptible. From the analysis of the urine after twenty-six hours, it seems probable that the magnesia had not then ceased to influence the amount of earthy phosphates.

(28.) Water of the same child as (6.) and (7.) 15·4 grains of magnesia taken about 7½ o'clock. Did not operate until after last water was passed. Food as before.

	Earthy phosphates.	Spec. gr.
11 to 1 o'clock, water alkaline	·62 per 1000 urine	1025·3
3 to 5 o'clock, water acid	1·57 per 1000 urine	1027·7

(29.) Same child. Food as before. No magnesia.

	Earthy phosphates.	Spec. gr.
2 to 5 o'clock, water acid	·36 per 1000 urine	1018·5

(30.) Same child. 19·3 grains of magnesia at 8 o'clock. Medicine acted about three o'clock.

	Earthy phosphates.	Spec. gr.
11 to 1 o'clock, water neutral	·45 per 1000 urine	1014·4
3 to 5 o'clock, water acid	·80	1017·1

The conclusions from these experiments are—

I. As regards variation in the phosphates.

The earthy phosphates soon after food were found to vary from 1·91 per 1000 urine, specific gravity 1033·2, to ·97 per 1000 urine, specific gravity 1027·3.

Long after food they vary from ·21 per 1000 urine, specific gravity 1028·2 to ·75 per 1000 urine, specific gravity 1028·0.

The alkaline phosphates long after food, and soon after exercise, vary from 8·10 per 1000 urine, specific gravity 1028·0, to 6·50 per 1000 urine, specific gravity 1022·8.

Long after food the quantity varies from 6·67 per 1000 urine, specific gravity 1025·5, to 4·72 per 1000 urine, specific gravity 1033·2.

II. As to the causes of the variation.

(*a.*) As regards food.

The earthy phosphates were not materially influenced by a diet of meat or of bread. They were in excess after either was taken; but on distilled water and meat alone, the excess was considerably below the average.

A long time after food the earthy phosphates were greatly diminished.

The alkaline phosphates were present in greatest quantity when bread alone was taken for food; when meat alone was taken, the deficiency was more marked than the excess with bread alone was. There was the most marked difference when the bread diet was compared with the meat diet.

(*b.*) As regards exercise.

Exercise produced no marked effect on the earthy phosphates.

On the alkaline phosphates exercise caused an increase of nearly one-third the amount previously excreted. This difference is not so great as that between bread and meat diets; so that probably though exercise has some influence, the kind of diet has a greater influence.

III. As to the effect of medical substances on the earthy phosphates.

(*a.*) As regards chloride of calcium.

15 grains of chloride of calcium produced no, or very little, effect in two hours and a half.

22 grains in rather more water produced a very decided increase in two hours and a quarter.

30 grains produced a still more marked increase in the same time, and the effect continued to be perceptible for ten hours.

(*b.*) As regards sulphate of magnesia.

30 grains of sulphate of magnesia in $1\frac{1}{2}\overline{3}$ water produced no, or very little, effect in two hours; in four hours and a quarter an increase was distinctly visible.

40 grains in $2\frac{1}{4}\overline{3}$ of water produced a very slight effect in two hours and a quarter; in $4\frac{3}{4}$ hours an increase was very distinct, and continued to be perceptible for nine hours.

(*c.*) As regards calcined magnesia.

45 grains of magnesia produced no effect in two hours and three quarters; in five hours there was a marked increase; in seven hours and a half a still greater increase, which was very marked at the end of twelve hours, and possibly continued for twenty-seven hours to influence the amount of earthy phosphates.

30·8 grains of magnesia produced no increase in two hours and a quarter; in four hours and three quarters the increase was very evident; and after eight hours and three-quarters it was still very marked; and after even twenty-six hours it still increased the amount of earthy phosphates in the urine.

These last experiments give the explanation of the rapid increase of phosphatic calculi, and of the enormous quantities of earthy matter discharged, when magnesia or lime-water have been taken in calculous affections. They show that these substances, having probably combined with different acids, pass off by the urine, and when this latter is alkaline react on the phosphate of soda, and thus increase considerably the amount of earthy phosphates in the deposit.

The result of these experiments is, that the amount of earthy phosphates precipitable by ammonia, depends chiefly on the amount of earthy matter taken into the body and that the amount of alkaline phosphates is also most chiefly influenced by diet yet that there is an additional cause constantly acting in the state of health, namely the production of phosphoric acid by the changes in the tissues of the body. And as in disease some of these tissues may be more particularly engaged, so then may the amount of alkaline phosphates point out the character, and declare the nature of the structure which is the seat of the affection.

On Alkalescence of the Urine from fixed Alkali.

The cases in which the urine is alkaline may be divided into two classes. In the one the alkalescence arises from volatile alkali, and in the other from fixed alkali. In the first it is caused by carbonate of ammonia, and in the second by carbonate of soda, or potash, or alkaline phosphate of soda. Decomposition of urea is the origin of the one, and disordered secretion of the other.

Whenever alkalescence arises, the earthy phosphates, whatever their quantity, are generally precipitated; and hence the expression phosphatic diathesis, a term which makes no distinction between the different kinds of alkalescence, nor between cases in which the earthy phosphates, sometimes far below their average quantity, simply appear in consequence of their insolubility in alkaline fluids, and cases in which a vast excess of earthy or alkaline phosphates is being excreted.

The object of the present paper is to point out the fact and the value of the distinction between the different kinds of alkalescence.

M. Pelouze has shown how rapidly decomposing mucus effects the conversion of urea into carbonate of ammonia. Irritation of the mucous membrane may give rise to mucus which produces this change, and in consequence the blue colour will be restored to reddened litmus paper if dipped into urine containing such mucus; or if

blue paper be used, this, whilst wet, will retain its colour; but if the test-paper be left to dry in either case it will be found that a change takes place. From the reddened litmus paper first used the blue colour will disappear, whilst the blue paper, when quite dry, will become red in consequence of a slight decomposition of the ammoniacal salt. This decomposition I have elsewhere shown to be the result of the evaporation of all ammoniacal solutions, and thus a ready and easy way is afforded of determining in any case of the alkalescence of the urine, whether it is caused by some ammoniacal salt, or whether it results from the presence of some fixed alkali*.

It not unfrequently happens that alkalescence is caused by fixed alkaline salts in those who, though not ill, yet suffer from indigestion whilst leading sedentary lives. I have more especially observed it where the octahedral crystals, usually supposed to be oxalate of lime, have been present. After a breakfast consisting chiefly of bread, in an hour and a half the water passed may be found healthily acid to test-paper, but that which is next passed, that is, from two to four hours after breakfast, will have an alkaline reaction. Frequently blue test-paper will be found, when dry, to undergo no change from the action of such urine. It will remain of nearly as deep a blue as before when the fluid has perfectly evaporated. This urine when passed will, though alkaline, often be perfectly clear, and if it be heated a granular precipitate will fall, the fluid becoming turbid from the deposit of earthy phosphates, which dissolve in dilute hydrochloric acid, usually without any effervescence.

Such a precipitation by heat takes place when the urine is not even neutral. It may be slightly acid. When boiled a precipitate falls, and if the fluid is then tested it is found to be more acid than before. If such a deposit from acid urine is left to become cold, the earthy phosphates are found to be partially, and sometimes even entirely redissolved, being again precipitable by boiling, and again partially or entirely dissolving on cooling.

If such urine as I have mentioned is passed alkaline and thick from deposit, it will be found, if immediately examined by the microscope, to be entirely granular (Plate V. fig. 2), the supposed form of phosphate of lime. Dilute hydrochloric acid, if added occasionally causes an effervescence, which in some cases arises from some alkaline carbonate in solution.

If the alkalescent or neutral urine is left for some hours, the surface becomes covered with an iridescent pellicle (fig. 1). This examined with the microscope contained here and there a long prismatic crystal, but the pellicle itself consisted of plates covered with spots of amorphous deposit. Some of these were triangular, some quadrilateral, some with regular and others with a ragged margin. The iridescence depended on these plates, which probably consist of phosphate of lime, as in some cases not a single prismatic crystal has been visible.

In some who suffer from indigestion the deposit of amorphous phosphate is con-

* This method, however, I have found to fail when much urate of ammonia and only a small quantity of fixed alkali chanced to be present.

stantly seen about three hours after breakfast, and very rarely at other times. In others the alkalescence of the urine is frequently observed, but the deposit is rare; whilst in others the deposit by heat from acid urine is very frequently to be found; and alkalescence is seldom to be detected by test-paper in the water secreted from two to four hours after food, and these three states often alternately occur in the same case.

Dr. ANDREWS of Belfast stated to me, that having observed a case otherwise in perfect health, in which the urine was almost invariably alkaline about two hours after breakfast, so much so as frequently to be loaded with a deposition of phosphates whilst still in the bladder, he was led to observe the urine of about fifteen students in good health immediately after it was voided about noon. He found it to be alkaline in about two-thirds of the cases. Whether this tendency to alkalescent urine may, as Dr. ANDREWS thinks, be connected with the immunity enjoyed by the inhabitants of his district from calculous affections, or whether alkalescence at this period of the day is far more general everywhere than has been supposed, future observations must determine.

At the present time I know five physicians in whom the above phenomena at this period of the day are more or less frequently visible in a greater or lesser degree; and in London this alkalescence will be found in those who are considered generally healthy much oftener than is imagined.

Supposing that acid phosphate of soda was the cause of the acid reaction of healthy urine, it was thought that some explanation of the deposit on boiling might be gained by observing the behaviour of phosphate of lime and phosphate of magnesia with phosphate and biphosphate of soda. Pure solutions of these salts and of chloride of calcium, and sulphate of magnesia were used, and the following results obtained. The deposits were examined with a magnifying power of 320 times.

Chloride of calcium gave no immediate precipitate with a strong solution of acid phosphate of soda. If left to stand many hours, a crystalline precipitate formed (fig. 3). When boiled no cloudiness was observed, if the quantity of phosphate of lime present was small; but if much was in solution, a crystalline precipitate fell on boiling; and when cold, if filtered and again boiled, a very small crystalline precipitate was occasioned, which did not entirely redissolve on cooling.

When a solution of biphosphate of soda is mixed with chloride of calcium, an immediate precipitate is caused by a drop or two of any alkali, and this is crystalline (fig. 3), or granular (fig. 2), or these mixed according to the quantity of alkali added, that is, according as much or little of the acidity of the solution is removed.

If this precipitate was separated by filtration and the clear liquid boiled, a deposit fell which was at first gelatinous (fig. 2), the fluid becoming more acid to test-paper. The precipitate, if the solution was very acid, changed into the crystalline form (fig. 3), and partly dissolved on cooling.

If chloride of calcium was added to common phosphate of soda, a plentiful granular

precipitate fell (fig. 2). This remained granular or changed into the crystalline form (fig. 3), according as the phosphate of soda was or was not in excess. If the chloride of calcium was added in excess, the fluid became acid to test-paper; the precipitate was at first gelatinous, but changed after some hours into crystalline, the fluid becoming less acid after some time. If phosphate of soda was in excess, the precipitate remained of a mixed granular and crystalline appearance, containing some crystals but more granular phosphate of lime (fig. 4).

If common phosphate of soda was poured drop by drop into chloride of calcium, a precipitate fell, which was at first gelatinous (fig. 2), the fluid becoming strongly acid to test-paper; if left to stand, the precipitate became crystalline (fig. 3), and at length lost some or all its acid reaction, which it reacquired again on boiling.

If common phosphate of soda was dropped into solution of nitrate of lime in excess, the result was the same gelatinous granular precipitate first forming, the same acid reaction, and the same change into the crystalline form on standing.

If any of these precipitates were separated by filtration, and the clear liquid boiled, a further slight precipitation occurred, which was granular (fig. 2). If the phosphate of soda was not in excess, the boiling caused an increase in the acid reaction of the liquid. The precipitate which falls when chloride of calcium is added to phosphate of soda, completely dissolves in solution of biphosphate of soda. Such a solution, if heated, gave a plentiful precipitate when boiled; this was granular, and partly dissolved on cooling; but if a great excess of biphosphate of soda was added, the precipitate was much less, and crystalline; and if filtered, but little precipitate again fell on boiling.

If sulphate of magnesia was added to a solution of biphosphate of soda, no precipitate fell; nor on boiling did any change occur. If but little alkali was added, no precipitation occurred on boiling; if rather more, a small, highly crystalline precipitate fell (fig. 5); if still more, heat threw down a plentiful gelatinous granular mass (fig. 2), which most rapidly dissolved on cooling.

If sulphate of magnesia was added to common phosphate of soda, little or no precipitate occurred, but if boiled a gelatinous precipitate fell; this dissolved as the fluid cooled. Under the microscope it was seen to be amorphous (fig. 2). If it was in such excess as not entirely to redissolve on cooling, a few drops of biphosphate immediately made the liquid clear. This, if boiled, gave a plentiful precipitate, and more quickly dissolved on cooling than before. If the liquid was very acid from biphosphate of soda, a slight crystalline precipitate fell, consisting of minute rhombic crystals, similar to those which were seen in the experiment with biphosphate of soda (fig. 5).

If a solution of phosphate of soda was dropped into an excess of sulphate of magnesia, after long standing a crystallization of small needles took place (fig. 6), but the fluid did not become acid to test-paper: nor if dropped into an excess of solution of chloride of magnesium was an acid reaction perceptible.

If to perfectly healthy and strongly acid urine a drop or two of a solution of chloride of calcium is added, no precipitate falls. If the acidity is lessened by any alkali, on boiling a granular precipitate is occasioned. This, when the urine is still acid, is partly or entirely redissolved on cooling; or if alkaline, immediately dissolves in dilute hydrochloric acid without any trace of effervescence.

Between three and five hours after food, at which time the earthy phosphates are always in excess, if healthy acid urine which gives no deposit on boiling has some of its acidity removed by fixed alkalies, or alkaline phosphate, a deposit takes place on boiling, and this is always granular, the fluid becoming more acid than before.

If to such alkaline urine as is passed thick from earthy phosphate a little biphosphate of soda is added, the phosphate redissolves; and if the biphosphate is not added in excess the earthy phosphates can be precipitated by heat, the reaction becoming more acid; but if an excess be added, the fluid remains perfectly clear on boiling.

There is then the closest coincidence between the deposits of earthy phosphates in some states of the urine, and their deposit from solutions of the phosphates of soda; and the same method which is followed for obtaining a precipitate of earthy phosphates dissolved in biphosphate of soda, will give a precipitate from healthy acid urine, and that which hinders precipitation in the one has the same effect on the other.

The deposit of phosphate of magnesia by boiling was supposed by M. Riffault to depend on the formation of a more basic phosphate of magnesia. The same explanation is still more probably the truth regarding the precipitation of the phosphate of lime by boiling, whether from solutions of phosphates of soda or from the urine.

The formation of crystalline earthy phosphates when great excess of earthy phosphate was present while at the same time the biphosphate of soda made the liquid very acid, gives the explanation why crystalline phosphate of lime is so seldom seen in the urine. Still it may occasionally be met with. Crystalline phosphate of magnesia, from its greater solubility, can scarcely appear. The amorphous deposit of phosphate of magnesia when urine is boiled may perhaps be recognised by its far greater solubility than the phosphate of lime as the fluid cools.

In the state of health acid phosphate of soda, mixed probably with common phosphate of soda, holds the earthy phosphates in solution. No precipitate is occasioned by chloride of calcium. If, after the water is passed or before from medicines, or particular food, or state of body, some of the acid phosphate is converted into common phosphate, a precipitate takes place on boiling the acid urine. If this very rapidly dissolves before the fluid is cold, the precipitate contains most probably phosphate of magnesia; if very slowly, it is more likely to be phosphate of lime. If the urine be neutral to test-paper, that is, contains still less biphosphate of soda (the common phosphate being decidedly alkaline to test-paper), then the precipitation

is more marked and the re-solution on cooling very much less. If the urine be alkaline, containing only common phosphate, this may be passed clear, and still may contain some phosphate of magnesia and a little phosphate of lime, these being somewhat soluble in common phosphate of soda, and these will be precipitated on boiling.

If the phosphate of lime is from any cause in great excess, it may be deposited as a granular deposit, and never in the crystalline form, unless it be in so great an excess that it is deposited from urine containing very much biphosphate of soda.

The occurrence of the alkaline condition at the particular period of the day which has been observed is well worthy of attention. The whole truth cannot be arrived at without a very lengthened inquiry into the variations in the amount of acids excreted by the kidneys, but partly at least it must depend on the food which has been taken in the morning, that is on the passage of alkaline phosphates, or carbonates, or salts of the vegetable acids through the system. Recent analyses of the ashes of seeds, flesh and blood, do not show any trace of alkaline carbonates, but as these cannot be heated to a red heat with common alkaline phosphates without the loss of carbonic acid, it will be seen how difficult it is to arrive at certainty on this point.

The conclusions from these observations are—

1. That there exist two kinds of alkalescence of the urine; the one long known as ammoniacal, the other not distinctly recognised, arising from fixed alkali. This last appears most frequently in water secreted from two to four hours after breakfast in persons suffering only from indigestion.

2. Water made at this period may be thick when passed from amorphous sediment, or it may be alkaline to test-paper, and still clear; or it may be free from deposit and slightly acid. If either of these last be heated, an amorphous precipitate may fall, which is soluble in dilute hydrochloric acid, or in solution of biphosphate of soda.

3. Healthy urine may at any time be made to give a precipitate of earthy phosphates by heat, even though it be acid, by having a little of its acid reaction removed by any alkali, or by common phosphate of soda, the fluid becoming more acid when boiled.

4. The solution of earthy phosphates in biphosphate of soda, gives also a precipitate on boiling if some of its acid reaction is removed by any alkali. The fluid when boiled becomes more acid to test-paper, indicating the formation of a more basic earthy phosphate.

5. A precisely similar result is obtained when common phosphate of soda, phosphate of lime, and a little biphosphate of soda exist in solution together; and by varying the quantities of each of these substances, the various phenomena which the urine occasionally presents may be produced at will.

6. The time at which the alkalescence of the urine from fixed alkali generally occurs, indicates the existence of some alkaline phosphate or of some carbonated alkali in the food*.

7. The result as regards diagnosis may be thus arranged :—

Alkalescence of the urine from local causes.	Alkalescence from general causes.
Blue paper made markedly red on drying.	Blue litmus paper not made red on drying.
Alkalescence constantly present.	Alkalescence variable, usually soon after food.
Always contains mucus in excess.	Rarely contains mucus in excess.
Prismatic crystals always to be found by microscope.	When first passed generally contains only granular deposit.

PLATE V.

Fig. 1. Iridescent pellicles on some alkaline urine.

Fig. 2. Amorphous deposit in alkaline urine.
Deposit on boiling phosphate of soda with chloride of calcium, or with sulphate of magnesia.

Fig. 3. Chloride of calcium with acid phosphate of soda, or with common phosphate of soda ; after long standing.

Fig. 4. Phosphate of soda with little chloride of calcium.
Bone-earth phosphate.

Fig. 5. On boiling phosphate of soda with sulphate of magnesia and little biphosphate of soda.

Fig. 6. Phosphate of soda with sulphate of magnesia ; after long standing.

Appendix.

Later experiments have shown that the alkalescence from fixed alkali does not depend on the nature of the food. For example, with a diet of animal food and distilled water, the urine in four hours has been observed to be alkaline. Rather longer after dinner it has also been found to be alkaline. Usually however, after a late dinner, even if the water is secreted alkaline, it becomes mixed in the bladder during sleep with acid water which is afterwards secreted and thus the alkalescence escapes notice.

It seems highly probable that the quantity of acid poured out into the stomach sets free alkali sufficient in some cases to make the urine alkaline; and from facts which have been stated to me, it seems even possible that the same effect on the water may sometimes be produced by the separation of acid by the skin.

* See Appendix.

XVI. *On the Gas Voltaic Battery.—Voltaic Action of Phosphorus, Sulphur and Hydrocarbons.*

By W. R. Grove, *Esq., M.A., F.R.S., V.P.R.I., Prof. Exp. Phil., London Institution.*

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In a paper which was honoured by publication in the Philosophical Transactions for 1843, I described certain forms of the gas voltaic battery, together with a series of experiments in which different gases were employed as voltaic combinations, and the consequent application of voltaism to eudiometry.

To ensure confidence in the accuracy of the eudiometric experiments, it was essential that the position which I laid down as to the absence of all voltaic action in a combination of oxygen and nitrogen should be rigidly true. I may state with certainty that it is so, but an apparent exception noticed (Exp. 21.) in my last paper obtains during the first few minutes after the circuit is closed, and sometimes for a much longer time. The examination of this temporary action in the first instance, with the view of ascertaining whether it was a specific action of the nitrogen or attributable to adventitious circumstances, led me to the results which I have the honour of laying before the Royal Society in this paper.

Before detailing these results, I will for convenience sake premise that they were all obtained by the form of battery represented in fig. 8 of my last paper (which, with a slight addition to be referred to presently, is again represented at fig. 2, Plate VI.), charged with distilled water slightly acidulated with pure sulphuric acid.

I will also, when alluding to my last paper, to avoid the needless repetition of the word *experiment*, refer to the number of the experiments as though they were paragraphs, and continue those numbers in the paragraphs of this paper.

As the form of battery (fig. 2) by which the interfering action of the atmosphere is entirely prevented was not devised until the greater part of the experiments in my last paper had been completed, I repeated some of them which seemed to require such verification with this battery; to one of these only is it essential that I should now refer. I should likewise mention, that in the experiments to be described the proper reductions for temperature and pressure have been made when necessary; where it was practicable the experiments were examined on days when the temperature and pressure were, as nearly as may be, the same as when they were set by.

(31.) Oxygen and deutoxide of nitrogen, which in the open form of battery gave only a temporary action (9.), when employed in the closed form (fig. 2) gave a continuous current. The following three sets of experiments were continued each for a

month in closed circuit, during which time they were constantly tested by the galvanometer and evidenced a continuous voltaic action; at the expiration of the month the results were as follows:—

Experiment 1.—Rise of liquid in tubes of		
	Oxygen	=0·32 cubic inch.
	Deutoxide of nitrogen. . .	=1·26 cubic inch.
Experiment 2.—In oxygen tubes.		=0·5 cubic inch.
	Deutoxide of nitrogen. . .	=2·5 cubic inches.
Experiment 3.—In oxygen tubes.		=0·2 cubic inch.
	Deutoxide of nitrogen . .	=0·75 cubic inch.
Mean result.	In oxygen tubes, rise . . .	=0·34 cubic inch.
	In deutoxide tubes, rise . .	=1·5 cubic inch.

The slight excess being undoubtedly due to the greater solubility of the deutoxide, it appears that four volumes of deutoxide of nitrogen are absorbed in the gas battery for one of the associated oxygen, and the result would be a compound of 1 equiv. nitrogen + 3 oxygen, or hyponitrous acid, which is exactly that formed by the slow combination of these two gases in the ordinary chemical way. The difference of amount of action in the three experiments depended on the temperature, the second experiment being made towards the close of last summer, the last experiment during the continuous cold weather of the present spring.

These experiments, coupled with the converse ones with hydrogen and deutoxide of nitrogen (30.), afford very satisfactory instances of the illustration of the law of definite combining volumes by the gas battery, exhibiting in one result, and itself registering that result, the action of equivalent chemical combination, catalysis and voltaism.

(32.) I now pass to the experiments which will form the more immediate subject of this paper. The temporary action to which I have alluded (21.) being greater when nitrogen and oxygen were the gases used, if the nitrogen were obtained by burning phosphorus in atmospheric air, than if procured from other sources, it naturally occurred to me that this action was due either to some phosphorous acid remaining in a state of vapour in the nitrogen, or to a slight portion of the phosphorus itself being held in solution in the nitrogen, as believed by Vauquelin and the older experimentalists. If this last supposition were the correct one, it seemed to offer a means of rendering phosphorus, though a non-conductor and insoluble in aqueous liquids, yet a permanent voltaic excitant analogous to the oxidable metals.

(33.) A small piece of phosphorus having been carefully dried, and weighing when dry 9·6 grains, was passed up through the liquid into the large tube of a gas battery by means of a small loop of mica, which kept it separated both from the glass and the platinum; the tube was now charged with pure nitrogen, and the associated tube with pure oxygen, the level of the gases or water-mark being noted by a little slip of paper pasted on the tube; a check experiment of oxygen and nitrogen without phos-

phorus was charged at the same time; the whole was carefully closed from the atmosphere and set by for twenty-four hours in closed circuit, to get rid of any current from adventitious circumstances; the next day, when tested by the galvanometer and iodide of potassium, a very decided action was apparent in the phosphorus battery, the iodide being decomposed and the galvanometer needle swinging round to 30°, the nitrogen with the phosphorus representing the zinc of an ordinary voltaic combination; the check experiment gave not the least deflection or decomposition. The experiments were suffered to remain in closed circuit for four months, from August 10th to December 14th, 1844, having been frequently tested in the interim, and the galvanometer always evidencing a continuous voltaic action in the phosphorus battery. On the 14th of December, the water in the oxygen tube having by its rise denoted the absorption of a cubic inch of oxygen plus the slight quantity 0·05 cubic inch of oxygen due to solution, as proved by comparison with the second battery, the experiment was examined; the result was as follows:—

Rise of liquid in oxygen tube 1 cubic inch. In nitrogen tube 0.
Original weight of phosphorus 9·6 grains.
Present weight of phosphorus 9·2 grains.

The battery was again charged in a similar manner, and put by on the 19th December 1844; the phosphorus weighed 2·8 grains. This, in consequence of the extremely cold weather which has prevailed almost without intermission from that time to the present period, proceeded much more slowly, and was not examined until May 17th, 1845, when the results were as follows:—

Permanent deflection of galvanometer 8°.
Rise of liquid in oxygen tube. 0·35 cubic inch. In nitrogen tube 0.
Weight of phosphorus =2·65.

Taking a mean of these two experiments, which in their relative results approximate more closely than I could have anticipated under the circumstances, we get 0·415 as the proportional weight of phosphorus lost for a cubic inch of oxygen. Now as 24 : 31·4 :: 0·34 : 0·444. The result of these experiments therefore leaves no doubt that phosphorous acid is the product of the voltaic action, as it is of the slow combustion of phosphorus in air. The experiment was repeated with distilled water; the action was at first very trifling, but increased every day, and the water gradually acquired an acid reaction.

No light was apparent in any part of the apparatus when examined in the dark, indeed the action was much too slow to render such an effect probable; though if subsequently by heat or other means I should succeed, as I hope, in producing light, it will be curious to observe in what part of the circuit the luminous effect in the voltaic combustion is perceptible.

A series of ten cells of phosphorus and nitrogen associated with oxygen were charged, and perceptibly decomposed water with platinum electrodes.

The result of the above experiments gives, I believe, the first instance of the employment of a solid, insoluble non-conductor as the excitant of a continuous voltaic current; it proves that the existence of diffused phosphorus in nitrogen, as noticed by the old experimentalists, is not a consequence, as was once believed, of a partial combustion, but of an effusion continuing as long as the previously diffused phosphorus is abstracted, and it gives the very curious result of a true combustion, the combustible and the '*comburant*' being at a distance; phosphorus burned by oxygen which is separated from it by strata both of water and gas, of an indefinite length. This result, arrived at by a progressive series of inductions, scarcely now appears extraordinary, but would have been in all probability listened to with incredulity if simply stated as a fact a few years ago. By the galvanometer we may also ascertain the rate of this very slow and minute chemical action; thus if by an apparatus, as above described, my galvanometer gives a deflection of 8 degrees, I know that the phosphorus is being consumed at the rate of the seven millionth part of a grain per minute.

(34.) The next step was to ascertain whether this action was peculiar to nitrogen or common to other gases; for this purpose, a day or two after the first experiment was set aside, the following were also made, and the dates and results were as follows:—

No. 1. Phosphorus suspended in protoxide of nitrogen associated with oxygen: weight of phosphorus 5·3 grains. Charged August 11th, 1844.

No. 2. Similar experiment, but without phosphorus.

Tested occasionally by galvanometer, the first battery gave invariably a small deflection, but less than in experiment (33.); the second gave no deflection.

Examined the 22nd April, 1845.

No. 1. Water risen in tube of oxygen 0·75 cubic inch.

In protoxide tube 1·7 cubic inch.

No. 2. Water risen in oxygen tube 0·1 cubic inch.

In protoxide tube 1·6 cubic inch.

Phosphorus weighed 5 grains, therefore loss =0·3 grain.

In this experiment the rise of liquid in the tubes containing protoxide was evidently due to the solubility of that gas, as it was very nearly equal in both the batteries, and the second gave not the slightest galvanometric deflection; the result gives 0·65 cubic inch of oxygen consumed by 0·3 grain of phosphorus, bearing nearly the same relative proportions as experiment (33.); the only difference between the action of phosphorus in nitrogen and in protoxide of nitrogen is, that in the former it is more rapid, as proved both by the galvanometric deflection and by the quantity of oxygen absorbed in a given time.

(35.) Charged August 11th, 1844.

No. 1. Phosphorus in carbonic acid gas associated with oxygen; weight of phosphorus 5·9 grains.

No. 2. Same without phosphorus.

Tested by galvanometer, No. 1. always gave a deflection, No. 2. none.

On the 3rd of December the carbonic acid gas in both batteries had been absorbed, and the liquid had reached the extremities of both tubes.

In the oxygen tube of

No. 1, rise of liquid =0·75 cubic inch. In No. 2, 0·05 cubic inch.

Phosphorus weighed 5·6 grains, the proportional weight was therefore 0·3 grain phosphorus to 0·7 cubic inch oxygen.

Here again the proportions came out just as in (33.) and (34.), the intensity of action being intermediate, less than the former and greater than the latter.

(36.) Charged 18th December, 1844.

No. 1. Phosphorus in pure oxygen associated with oxygen, great care being taken to exclude atmospheric air: this arrangement having been kept in closed circuit for twenty-four hours, gave a very feeble deflection of the galvanometer.

Examined 15th February, 1845. The rise of liquid in the tube containing phosphorus was equal to 0·3 cubic inch, in that containing the associated oxygen = 0·05. I find no note of the phosphorus being re-weighed; probably I considered it useless, as the consumption of oxygen in the associated tube was so very trifling, scarcely sufficient to be distinguished from the effect of its solubility.

(37.) Charged 23rd April, 1845.

No. 1. Phosphorus suspended in deutoxide of nitrogen associated with oxygen; weight of phosphorus = 4·3 grains.

No. 2. Same without phosphorus.

Examined May 27th, 1845. Galvanometer gave 25° permanent deflection in No. 1, and 10° in No. 2.

No. 1. Rise of liquid in deutoxide tube =0·7 cubic inch.
Rise of liquid in oxygen tube =0·6 cubic inch.

No. 2. Rise of liquid in deutoxide tube =0·7 cubic inch.
In oxygen tube = 0·2 cubic inch.

Weight of phosphorus 4·17 grains.

Consequently had lost 0·13 grain for 0·4 cubic inch of oxygen.

In this and all the preceding experiments the residual gases were unchanged in quality, and in this experiment it appears that the action of the deutoxide of nitrogen and the oxygen was perfectly unaffected by the phosphorus, the consumption of deutoxide of nitrogen being exactly the same in both batteries. In another experiment, which I did not record on account of a minute bubble of air having entered the tubes containing the deutoxide, the phosphorus appeared to have exercised a retarding influence on the voltaic combination of deutoxide of nitrogen and oxygen; this I attributed to a slight deposit of phosphorous acid upon the platinum, by which its catalytic power was deteriorated.

(38.) It thus appears that the effect we have been examining, of the diffusion of

phosphorus in gas, is not due to any peculiarity of nitrogen, and is not peculiar to any particular gas, as once believed; but being in all probability common to all gases which do not exercise a specific action on the phosphorus, it may be more properly called a volatilization of phosphorus at ordinary temperatures than a solubility in gas; the ordinary slow combustion of phosphorus in air is, in fact, a combustion of its vapour. I incline to think that the inferiority of its vaporization in pure oxygen is due to a protecting film being formed, and that the phenomenon is in some respects analogous to the inactivity of iron in nitric acid.

(39.) Phosphorus in nitrogen was associated with hydrogen in the gas battery to ascertain their voltaic relation; the hydrogen was positive to the phosphorus, *i. e.* represented the zinc of an ordinary voltaic combination.

(40.) To realize the curious novelty of two non-conducting solids forming the elements of a voltaic battery, and producing a continuous current, phosphorus suspended in nitrogen in one tube of a gas battery was associated with iodine in nitrogen in the other; a very decided current was the result, which continued for months, the nitrogen remaining unaltered in volume, but the liquid becoming gradually tinged by the excess of iodine vapour. The following is the result of the experiment:—

Charged January 1, 1845.		Examined May 17, 1845.	
Weight of iodine . .	=5·9 grains.	Weight of iodine . .	=4·6 grains.
Weight of phosphorus.	=6·4 grains.	Weight of phosphorus.	=6·28 grains.

The phosphorus has consequently lost 0·12 grain, the iodine 1·3. Assuming that the phosphorus consumed 3 equivalents of oxygen, as in experiments (33.), (34.), (35.), (37.), we should have 3 equivalents of hydrogen eliminated, and consequently 3 of iodine consumed, or

$$31{\cdot}4 : 126{\cdot}6 :: 0{\cdot}12 : 0{\cdot}48$$
$$0{\cdot}48 \times 3 = 1{\cdot}44.$$

The result is tolerably near, but from the iodine vapour in solution an excess and not a deficit in the consumption of this was to have been expected.

(41.) It was necessary for my own satisfaction to make a great number of other experiments for the purpose of checking and eliminating any adventitious results which might possibly interfere with the actual voltaic action of the gas battery, such as placing phosphorus in single tubes containing the different gases, but with platinum foil and without associated tubes, others without the platinum foil or associated tubes; but as these had no influence on the results, and were merely used as tests for my own satisfaction, it would be useless and tedious to detail them.

(42.) Having examined the action of phosphorus in the gas battery under these various circumstances, my next step was to ascertain if any other substance produced a similar effect. Sulphur, the nearest analogue of phosphorus, was the body which naturally presented itself, but from its different characteristics required a different mode of manipulation. The following was adopted. Into a little capsule of glass,

having a long solid leg (see fig. 1), was placed a small piece of solid sulphur; this was held in the large aperture of a gas battery cell, while the tube was passed carefully over it; the platinum in this tube was connected with the zinc of a single cell of the nitric acid battery, while an anode of platinum was placed in the liquid through the central aperture; by this means all the oxygen of the atmospheric air was exhausted, and the surplus hydrogen was in turn taken away by connection with the associated tube charged with oxygen; the same effect might have been more slowly produced by the process described (29.). The sulphur was now in an atmosphere of pure nitrogen, and this could have been effected in no other way that I am aware of without wetting or forming some deposit on the sulphur. Having connected it in closed circuit with the oxygen tube for twenty-four hours, the galvanometer gave no deflection. A small hoop of iron with a handle was now heated and passed over the tube containing sulphur and nitrogen, the wires being connected with the galvanometer (see fig. 2). The result was very striking: I had directed my assistant to watch the galvanometer while I attended to the manipulation. At the same instant he exclaimed that the galvanometer was deflected, and I that the sulphur was melting; the galvanometer continued deflected during the whole time that the sulphur remained fused, and indeed some time afterwards, until all the sulphur vapour diffused in the nitrogen had become exhausted. The sulphur represented the zinc of an ordinary voltaic combination. It was of course impracticable in this case to ascertain the equivalent consumption. This experiment very strikingly exhibits the analogy of sulphur with phosphorus, and proves that the instant sulphur is fused it becomes a volatile body, as phosphorus is when solid; the suddenness of its action, coupled with the insoluble character of sulphur, leads to the conclusion that solution in the electrolyte is not a necessary antecedent to voltaic action in the gas battery. Indeed this might have been deduced from the experiments with phosphorus, as its vapour must have been nearly, if not absolutely insoluble in the electrolyte, or the equivalent results would not have come out so accurately; possibly solution and electrolysis are in these cases synchronous.

(43.) I was now led to try in the gas battery other substances differing from phosphorus and sulphur, but possessing characters which had hitherto prevented their being used as voltaic excitants; as, if my view of the volatility of phosphorus and sulphur were correct, other volatile bodies ought to act similarly. Camphor was the first substance I experimented on. A piece of camphor weighing 12·9 grains, was placed in a similar manner to the phosphorus (33.) in nitrogen, and associated with oxygen; tested by the galvanometer it gave a feeble deflection, which, however, was continuous; it was allowed to remain four months in closed circuit; at the expiration of that time the liquid had risen in the oxygen tube 0·3 cubic inch; the nitrogen in which the camphor was suspended had increased in volume 0·15 of a cubic inch. The camphor weighed 11·4 grains, but some minute crystals of it were observed at the top of the tube, so that the loss of weight was greater than that due to voltaic action.

(44.) The smallness of the quantity of the gas which had been added to the nitrogen, precluded an accurate analysis of it; enough was ascertained, however, to lead me to believe that it was hydrocarbonous, and it then became my aim to produce it in greater quantities. I attached a piece of camphor to a platinum wire, and to the same wire I also attached a piece of sponge platinum; I passed these up into a tube of nitrogen over distilled water, and at the expiration of three months the gas had increased 0·1 cubic inch; this proved that the camphor vapour was decomposable by the catalytic action of platinum at ordinary temperatures, and that the effect in the nitrogen cell of the battery was not due to its voltaic association; but the experiment did not give me a sufficient quantity of the gas for analysis.

(45.) I therefore had recourse to the apparatus, fig. 3. *a* is an inverted cylindrical test-glass; *b* a platinum capsule with a pin-hole in the bottom for drainage, standing on an ivory pedestal; *c*, *c* two very stout platinum wires; *d* a coil of fine platinum wire. Into the capsule *b* was placed the camphor, the glass *a* filled with distilled water was inverted over it and charged with pure nitrogen, to a level marked somewhere below the capsule; the wires *w w'* are now connected with a voltaic battery of sufficient power fully to ignite the wire *d*.

(46.) After the wire was ignited the volume of the gas gradually increased; when the original volume was doubled, the gas was examined. It had a strong disagreeable odour, very similar to that of coal-gas; it burned with a blue flame, slightly tinged with yellow: placed in an eudiometer, such as I formerly described*, and mixed with hydrogen, it underwent no alteration. Two volumes of it, mixed with one volume of oxygen, contracted one-sixth of the whole volume, and subsequently agitated with lime-water, contracted two-sixths more, lining the tube with a crust of carbonate of lime. The residual gas was nitrogen. It was thus clear that the vapour of camphor was decomposed by the ignited wire into carburetted hydrogen and carbonic oxide, and the analogy is too direct to leave any doubt that these gases were also formed in experiments (43.) and (44.) by the influence of the platinum foil and spongy platinum.

The apparatus (fig. 4) offers a most convenient means of decomposing volatile hydrocarbons, and possibly other substances.

(47.) Portions of oil of Turpentine and of Cassia were now placed in capsules (fig. 1), weighed and exposed each to an atmosphere of nitrogen in the large tube of a gas battery, by the same means as described (42.); they gave a very decided deflection (the nitrogen representing zinc). This deflection continues, and the liquid is slowly rising in the oxygen tubes, but the rise is too slight at the time of my writing this paper to derive any useful result from examining the present weight†.

* Philosophical Magazine, August 1841, p. 99, and Philosophical Transactions, 1843, p. 105.

† Dec. 1845.—The rise of liquid has been slow but continuous, and the galvanometer feebly deflected. In the Turpentine experiment the rise is =0·7 cubic inch, in the Cassia 0·5; the weights, however, from the irregularity of absorption and evaporation, give no data as to the equivalent consumption; thus the Turpentine has lost 0·7 grain, the Cassia gained 0·05 grain.

(48.) Alcohol and ether were tried in a similar manner, and produced notable voltaic effects; alcohol the most powerful probably, on account of its greater solubility in water.

(49.) The rationale of the action in experiments (43.) and (47.) is curious. It seems that the platinum in the nitrogen tube first decomposes the vapour of the hydrocarbons*, and then the same platinum, with its associated plate, recombines the separated constituents with oxygen. In experiment (43.) the decomposition takes place more quickly than the recomposition, as indeed would be expected from the absence of the resistance of the electrolyte in the former case, and hence the increase of gas in the nitrogen tube.

(50.) The analogy of the action of the above volatile substances strengthens the position advanced (38.), that solid phosphorus should be regarded as volatile at ordinary temperatures, and sulphur when fused; the whole of these experiments also serve to introduce the galvanometer as a new and delicate test, and in some cases a measurer of volatilization.

(51.) As the gas battery was shown in the former paper, which I had the honour to communicate to the Society, to give us the power of introducing gases which had been previously untried as voltaic excitants, and to ascertain their electro-chemical relations, it has, by the means detailed in this paper, opened a field for ascertaining the voltaic relations and quantitative electro-chemical combinations of solid and liquid substances, which from their physical characteristics had not hitherto been recognised in lists of the voltaic relations of different substances, and consequently formed to a certain extent a blank in the chemical theory (may we not now call it law?) of the voltaic pile. These results, coupled with the previously-arranged tables of electro-chemical relations, and with the great improvements in apparatus for measuring these relations recently made by Mr. WHEATSTONE and others, offer every promise of the ultimate establishment of accurate measures of affinity. I give the following tables as an approximative list, without attempting to give the degrees of intensity, which can only be filled up by a careful series of researches exclusively devoted to this object.

(52.)

Chlorine.	Camphor.
Bromine.	Essential oils.
Iodine.	Olefiant gas.
Peroxides.	Ether.
Oxygen.	Alcohol.
Deutoxide of nitrogen.	Sulphur.
Carbonic acid.	Phosphorus.
Nitrogen.	Carbonic oxide.
Metals which do not decompose water under ordinary circumstances.	Hydrogen.
	Metals which decompose water.

* I use this word here and in the title to avoid periphrasis; it is not quite correct as applied to some of these bodies.

Though carbonic acid and nitrogen appear to be neutral, and consequently might be bracketed with the metals which do not decompose water, as forming the nodal point or zero of the table, yet, in consequence of the peculiar action exercised by them, and detailed (29.) and (30.), I have placed them above the metals*.

(53.) The results embodied in my present and my former paper, I think sufficiently indicate the field of research opened by the gas battery, a field which may of course be indefinitely extended. I have never thought of the gas battery as a practical means of generating voltaic power, though in consequence of my earlier researches, which terminated in the nitric acid battery, having had this object in view, I have been deemed by some to have proposed the gas battery for the same purpose; there is, however, a form of gas battery which I may here describe, which, where continuous intensity or electromotive force is required, but the quantity of electricity is altogether unimportant, appears to me to offer some advantages over any form of battery hitherto constructed, and which, independently of any practical result, is, from circumstances peculiar to the gas battery, not without interest. It is shown at figs. 4 and 5. A A′ is a long glass tube, with a series of legs or glass tubes attached to and opening into it; the lower extremities of these are open, and the main tube or channel A A′ terminates at the extremity A in a glass stopper, and at A′ opens out into a funnel, as shown in the figure. Into a series of glasses B B′ are cemented platinum wires having attached to them strips of platinized platinum foil, two to each glass, the one being four inches long and half an inch wide, the other $1\frac{1}{2}$ inch long by one inch wide; the former set are placed lower than the latter, so that when the glasses are filled with liquid the former set shall be just covered, and the latter bisected by the water-mark; the last glass B has no platinum. These platinum strips are connected metallically by external wires, the narrow platinum of one cell with the wide one of the next, and so on in series. The glasses having been filled to the top of the narrow platinum with acidulated water, let a piece of zinc be placed on a pedestal in the vessel B, and the stopper being out of the extremity A, the apparatus A A′ lowered into the glasses, the tubular legs covering each one of the narrow platinum plates. The tubes will of course be full of water, and the main channel full of atmospheric air; this will gradually be displaced by the hydrogen ascending from the zinc, which hydrogen, in consequence of the curve at A, will retain its position. When it is judged that the greater portion of air has been expelled, the stopper at A, covered with a little grease, is to be inserted; the hydrogen now will rapidly descend in all the tubes until the zinc is laid bare, and then remain stationary.

We have now a gas battery, the terminal wires of which will give the usual voltaic effects, the atmospheric air supplying an inexhaustible source of oxygen, and the hydrogen being renewed as required by the liquid rising to touch the zinc; by supplying a fresh piece of zinc when necessary, it thus becomes a self-charging battery,

* I have been lately much struck with the difficulty of reconciling the theory of Grotthus with many of the combinations in the gas battery, and have stated this difficulty in the Philosophical Magazine for Nov. 1845.

which will give a continuous current; no new plates are ever needed, the electrolyte is never saturated, and requires no renewal except the trifling loss from evaporation, which indeed is lessened, if the battery be in action, by the newly composed water. There is an aperture in the pedestal with a moveable slide, through which the vessel B′ can be removed, when necessary, to replace the zinc, and the remaining part of the apparatus is never disturbed. This battery would form an elegant substitute for the water battery; it would much exceed in intensity a similar number of series of that apparatus; it would be applicable to experiments of slow crystallization and possibly to the telegraph. Its construction is difficult and makes its prime cost expensive, but after that it is the most durable, the most easily charged, and the most free from local action of any known form. I have had one of ten cells constructed, shown at fig. 5, which succeeds perfectly, giving sparks, decomposing water, &c., and is ever ready for use. Any number of such sets might be united by adapter-tubes; or indeed it would be much more economical, and reduce to a minimum the damage from breakage, to have the main channels A A′ made of varnished wood or porcelain, with apertures into which separate glass tubes might be cemented.

London Institution, May 30, 1845.

XVII. *On the Compounds of Tin and Iodine.* *By* THOMAS H. HENRY, *Esq.* *Communicated by* R. PHILLIPS, *Esq.*, *F.R.S.*

Received March 31,—Read June 19, 1845.

IN a paper by Sir H. DAVY, published in the Philosophical Transactions, 1814, he describes the compound of tin and iodine procured by heating these bodies together, out of the contact of air, as of a deep orange colour, fusible at a moderate heat, and volatile at a higher temperature.

The compound obtained by GAY-LUSSAC, by gently heating tin with twice its weight of iodine, and more recently by RAMMELSBERG, by the same method*, is described by them as a reddish-brown, transparent substance, yielding a powder of a dirty orange-yellow colour, and easily fusible.

A compound of tin and iodine was procured by BOULLAY†, by precipitating a solution of protochloride of tin with iodide of potassium, in slight excess.

The combinations procured by these methods have been considered to be identical in composition, although the compound procured both by GAY-LUSSAC and by RAMMELSBERG is stated by them to be decomposed by water, while the salt of BOULLAY is described by him as soluble in water without decomposition.

It will be seen, I think, from the following experiments, that the substance procured by heating tin with twice its weight of iodine, is a mixture of two salts, differing in composition, one of which is soluble in water to a slight extent, without suffering decomposition, while the other is immediately decomposed on bringing it in contact with water, the former being the real protiodide described by BOULLAY, and the latter a biniodide, a salt which has not yet, to my knowledge at least, been described, but which must have been the compound mentioned by Sir H. DAVY, as it is of a brilliant orange colour, and sublimes at a temperature of 356° FAHR., while the protiodide, I find, may be heated to redness, out of the contact of air, without subliming.

100 grs. of tin, in a state of minute division, were mixed with 220 grs. of iodine, the mixture placed in a porcelain crucible well-covered, and sufficient heat being applied to fuse the iodine, violent action immediately took place, accompanied by the evolution of much heat and the sublimation of a portion of the iodine; when the action had ceased and the crucible had become cool, a brown transparent crystalline mass, weighing 310 grs., was removed from it; 10 grs. of iodine had been sublimed, therefore, by the heat evolved during the combination; upon breaking this mass, however, a button of metallic tin, weighing 45 grs., was found inclosed in it. This

* Gmelin, Handbuch, vol. iii.

† Annales de Chimie, xxxiv. 372.

substance could not be the neutral compound, for the 210 grs. of iodine would require 98 grs. of tin to form the protiodide instead of only 55 grs.

The mass was therefore heated again in a Florence flask well-corked, with 45 grs. of tin in very fine powder, to replace the button removed from it, in order to ascertain whether the neutral compound could be procured by digesting this substance with the metal in a state more favourable to combination. The mass readily fused without any further action on the tin; but an orange-red sublimate was formed, condensing on the sides of the flask in brilliant acicular crystals. As the mass in the flask diminished in quantity, it became less fusible, until at length it required a degree of heat little short of dull redness to produce that effect, and then it ceased to give off vapour. The flask was now allowed to cool; when cold, it was cut, and the fused residue removed, which was found to weigh 86·5 grs. after the separation of 37·5 grs. of tin still uncombined.

This substance was of a deep red colour and crystalline texture, affording a powder of a bright red colour similar to that of minium; 50 parts of it were treated with strong nitric acid, which acted violently upon it, expelling iodine and leaving peroxide of tin, which weighed after ignition 22·1 parts = 17·38 metallic tin = 34·76 per cent.; a compound of one equivalent of tin = 59, and one of iodine = 126, would give 31·89 per cent. This excess in the quantity of tin arose from the heat employed in separating the two compounds, producing a portion of oxide by the decomposition of the protosalt, as will be seen further on. To ascertain the composition of the sublimate, 50 parts of it were decomposed by nitric acid, and gave 12·2 parts of peroxide = 9·597 metal = 19·19 per cent.

100 parts, treated with a solution of pure carbonate of potash in slight excess, were decomposed, carbonic acid being evolved and peroxide of tin precipitated; the iodide of potassium produced was separated by alcohol, and after dilution with water, was treated with nitrate of silver; the precipitate dried and fused weighed 148·5 = 79·99 iodine. This gives

		Theory.	Experiment.
2 Iodine	= 252	81	79·99
1 Tin .	= 59	19	19·19
	311	100	99·18

It was therefore a biniodide.

In the next experiment, I took one atom of each substance, viz. 59 grs. of tin and 126 grs. of iodine. The action was violent as before; there were 16 grs. of tin uncombined, and after the sublimation of the biniodide, the fused protosalt weighed 66 grs.; this was exposed to the air as little as possible during the process of sublimation, which being performed in a retort, less oxide was formed in consequence.

50 parts gave 21·1 peroxide of tin = 33·2 per cent. metal. In order to ascertain the action of heat upon the protiodide of tin, I prepared some by precipitating a warm concentrated solution of recently prepared protochloride of tin, by a strong solution of iodide of potassium, in slight excess; the salt formed on cooling in beau-

tiful acicular crystals, which, after being washed with a little water, lost their lustre by drying at a very gentle heat.

50 parts gave 20·7 parts peroxide =32·36 per cent. tin; 10 grs. of this were heated in a small tube, tightly corked, at first gently, and afterwards to complete fusion; a little water condensed on the upper part of the tube with a minute portion of biniodide. When cold the tube was cut, the fused mass removed, and found to have lost 1·5 gr.; it was slightly oxidized on the surface, and perfectly resembled the protiodide procured in the former experiments.

25·3 grs. of the same salt were heated to from 380° to 400° in an open porcelain crucible, a sublimate was produced, which was received in a paper cone, so placed on the crucible as not to prevent the access of air; when no more vapour was given off, the crucible was cooled and weighed; the residue was found to weigh 6·04 grs.; it was ignited, and then weighed 5·91 grs., and was peroxide of tin. The sublimate, which was in small brilliant orange-red crystals, was biniodide of tin; for 4·60 grs., decomposed with nitric acid and ignited, gave 1·115 peroxide = 19·06 per cent. of metal.

Now, supposing that two atoms of protiodide of tin had been decomposed, giving rise to one atom of periodide, and one atom of peroxide of tin, 25·3 grs. of protiodide should have left 5·13 grs. of peroxide, which is sufficiently near the quantity obtained to determine the nature of the decomposition. Berzelius states that the protofluoride of tin is converted, by the action of the atmosphere, into SnF^2, SnO^2; a decomposition exactly analogous to that above described.

I have not succeeded in obtaining a combination of tin and iodine corresponding to the sesquioxide, although Boullay conjectured that some yellow crystals, which he obtained on adding to a solution of protochloride of tin a solution of iodide of potassium, in which an additional half-atom was dissolved, were sesqui-iodide; the crystals I obtained by this method were found to be pure protiodide.

On adding iodine to a solution of protochloride of tin, this salt suffers a remarkable decomposition; if its solution be concentrated, an iodide of tin is precipitated, and a combination of chloride and iodide of the metal, in definite proportions, remains dissolved in the solution. If the iodine be added in excess, so that the solution acquires a brown colour, it yields crystals of biniodide on evaporation; if, on the other hand, the protochloride be in excess, a portion of the protiodide is precipitated, and the remainder unites with protochloride of tin, in the proportion of one equivalent of each substance, and it remains dissolved in the solution of protochloride, but may be separated by evaporation in the form of delicate acicular crystals of a silky lustre, and straw-yellow colour.

On adding iodine to an equal weight of protochloride of tin, dissolved in a small quantity of water, I obtained some minute red crystals, which yielded on analysis

	Per cent.
Iodine	79·30
Tin	19·98
	99·28

it was the biniodide therefore; with 380 grs. of the protochloride of tin and 150 grs. of iodine, a precipitate was obtained, which was found to yield—

	Per cent.
Iodine	67·78
Tin	31·67
	99·45

it was therefore protiodide of tin.

The solution remaining after the precipitation of the protiodide was evaporated at a gentle heat, until sufficiently concentrated, and allowed to cool; some crystals were thus obtained which were freed as much as possible from the mother-liquor, by pressure in bibulous paper, but on attempting to purify them for analysis by redissolving them in water, they were immediately decomposed, giving a scarlet crystalline precipitate of protiodide of tin, while chloride of tin remained in solution; they were therefore pressed as dry as possible in bibulous paper, and afterwards retained *in vacuo* over sulphuric acid for some hours.

10 grs. were then treated with a solution of pure carbonate of potash, evaporated to dryness, redissolved in cold water and separated from the protoxide of tin by filtration; the solution acidulated with nitric acid, and the iodine precipitated by nitrate of palladium while hot, the precipitate washed, dried, and ignited, left 1·94 gr. of metallic palladium =4·586 grs. iodine. The chlorine was afterwards precipitated by nitrate of silver; the precipitate, washed, dried, and fused, weighed 5·22 grs.; upon dissolving the chloride of silver in ammonia 0·1 gr. of metallic palladium was separated, which had subsided with the chloride of silver as a subsalt; this leaves 5·12 for the true weight of the chloride of silver =1·263 chlorine; 5 grs. decomposed with nitric acid in excess, evaporated to dryness and ignited, gave 2·68 grs. peroxide of tin =2·108 of metallic tin.

This gives in 100 parts—

	Theory.	Per cent.	Experiment.
One atom chlorine =	35·4	or 12·67	12·63
One atom iodine =	126	or 45·10	45·86
Two atoms tin . . =	118	or 42·23	42·16
	279·4	100·00	100·65

It is therefore a compound of one atom of protochloride of tin, and one atom of protiodide of tin.

The excess in the quantity of iodine in the above analysis, is probably owing to the tendency of the protonitrate of palladium to subside as a basic salt in company with precipitates, which is a great objection to its employment in analysis.

As the protochloride of tin is stated by GMELIN*, on the authority of BERZELIUS, to contain one atom of water of crystallization, while the late Dr. TURNER, in the last edition of his Elements, published during his life†, states that it contains three atoms

* Handbuch, vol. iii. † Edit. v. p. 550.

of water of crystallization, and several works of character do not give the composition of the crystallized salt at all, it became necessary to analyse the salt used in the above experiments; it was obtained by digesting strong hydrochloric acid with tin in excess at a moderate heat, and when a tolerably concentrated solution was obtained, decanting it and setting it aside to crystallize; the strongly acid mother-liquor was again digested with the tin and took up a fresh portion (it appears impossible to saturate hydrochloric acid with tin at once). The salt was in small prismatic crystals and dissolved in water, after drying on blotting-paper, forming a perfectly clear solution, and producing a great degree of cold; 700 grs. dissolved in 3 oz. of water reduced the temperature from 58° to 27° FAHR.

The crystals were coarsely powdered and pressed as dry as possible in bibulous paper; 25 grs. were dissolved in water acidulated by sulphuric acid, and the tin precipitated by sulphuretted hydrogen, the protosulphuret washed and dried, weighed 17·23 grs.; 16·2 of these were ignited in a porcelain crucible, and the ignition, repeated with a little carbonate of ammonia, gave 15·6 grs. of peroxide; this corresponds to 16·59 on the total quantity of sulphuret, which is equivalent to 13·09 of metal.

The excess of sulphuretted hydrogen was removed by a little sulphate of copper, and the chlorine, precipitated by nitrate of silver, gave 30·94 grs. of fused chloride of silver =7·72 chlorine.

To determine the water, 23·58 grains were retained *in vacuo* over sulphuric acid for twenty-four hours, and were found to have lost 3·87 grains = 16·41 per cent.; we have therefore—

	Experiment.	Theory.	Atom.	
Chlorine . . .	30·88	31·50	1	35·4
Tin	52·36	52·49	1	59
Water. . . .	16·41	16·01	2	18
	99·65	100·00		112·4

The following table contains the principal analytical results of this communication:—

Protiodide of tin . . .	SnI, solid and fixed, sparingly soluble.
Periodide of tin . . .	SnI_2, solid, volatile, decomposed by water.
Chloriodide of tin . . .	SnCl, SnI, solid, fixed, decomposed by water.

March 28, 1845.

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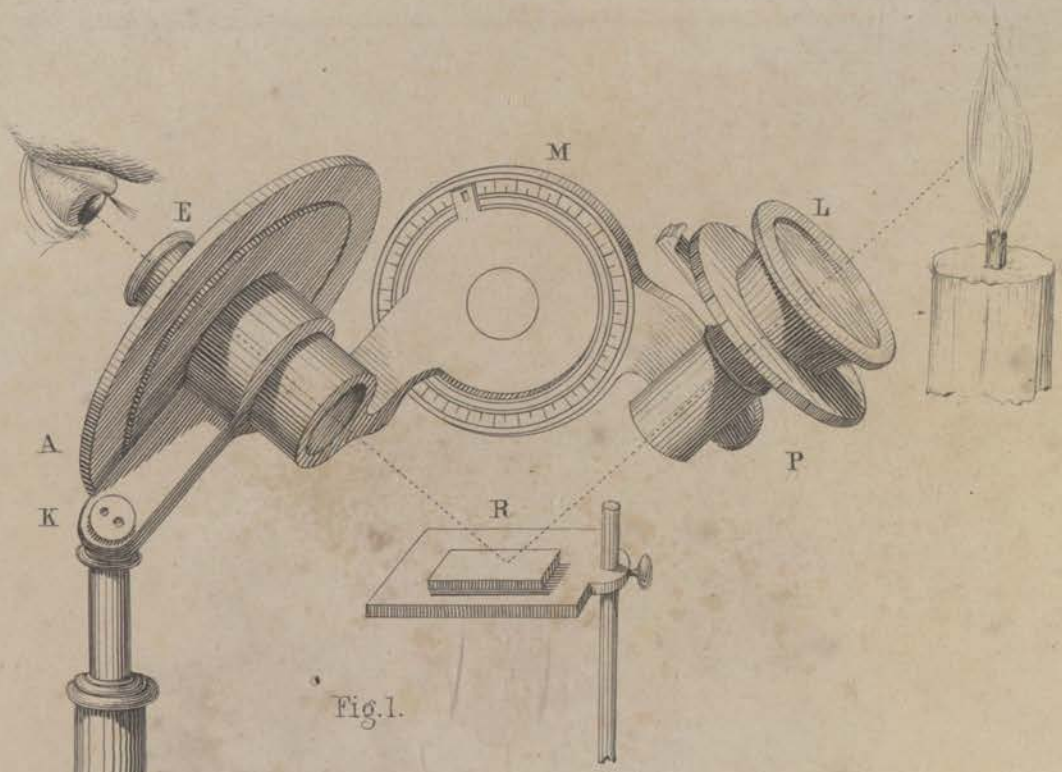

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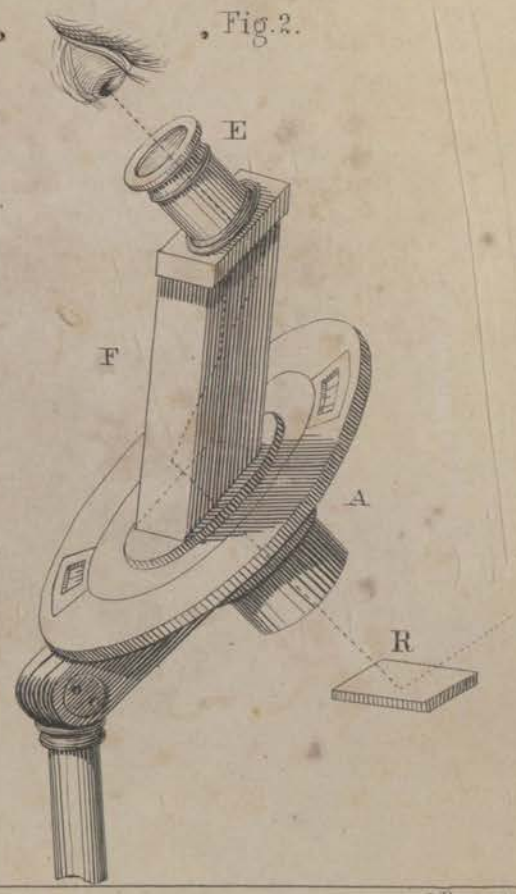

Fig.3.

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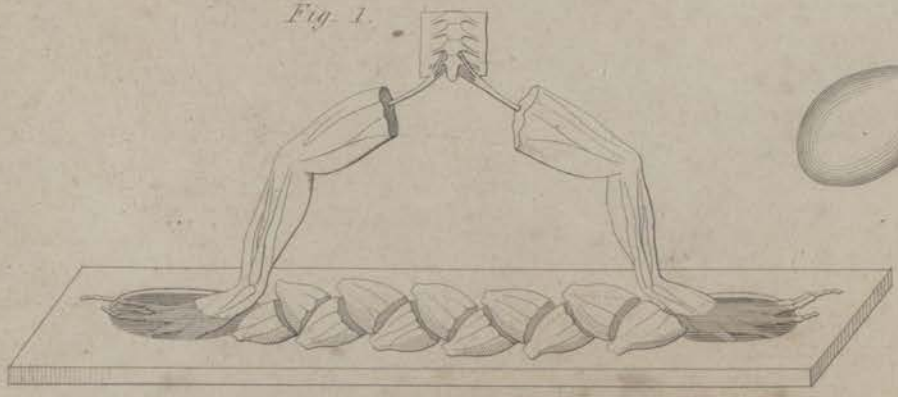

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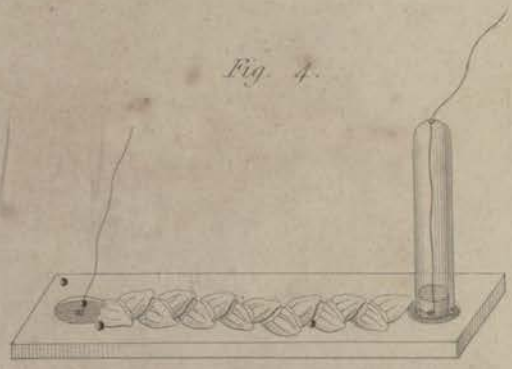

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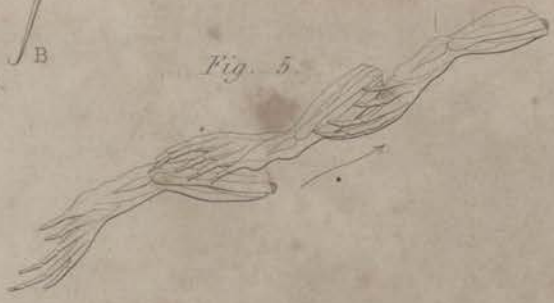

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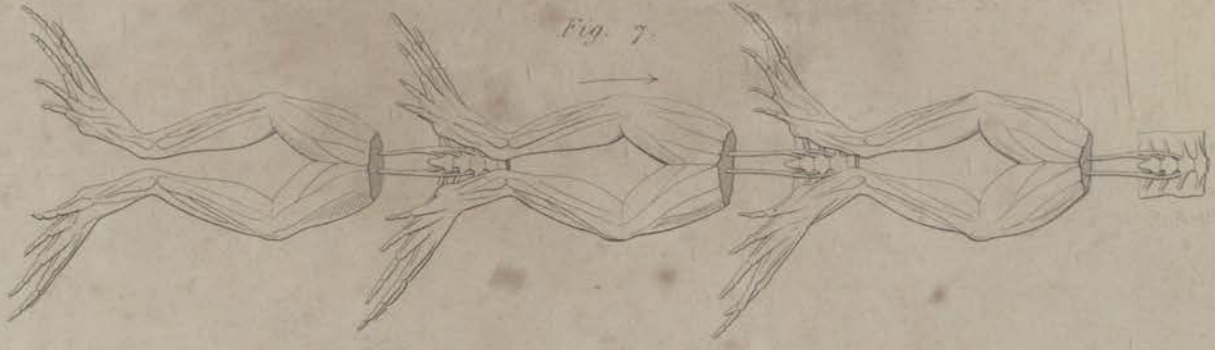

Fig. 8.

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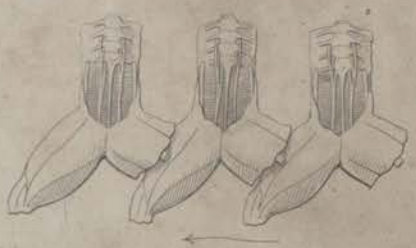

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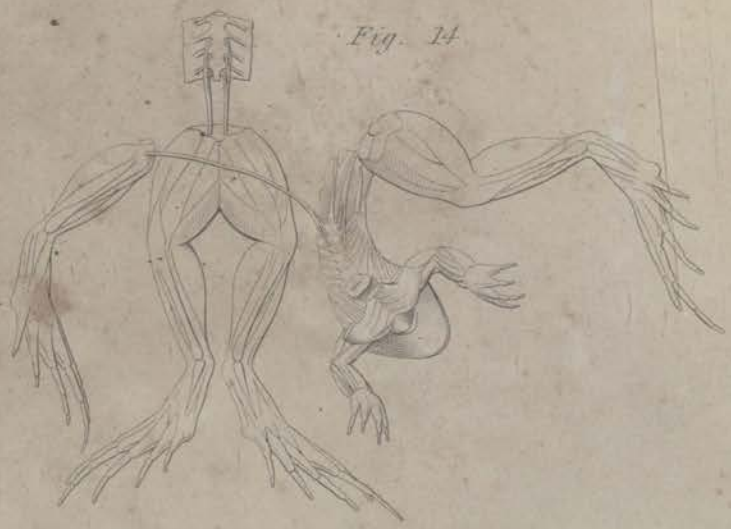

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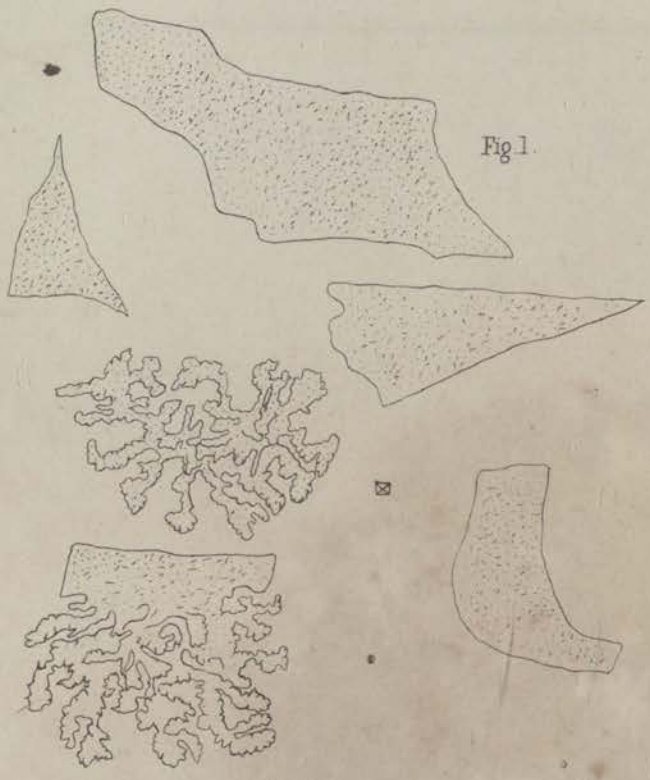

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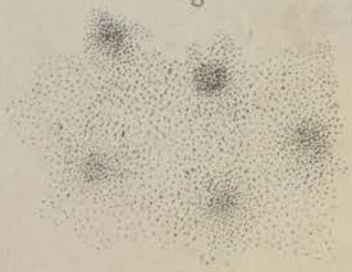

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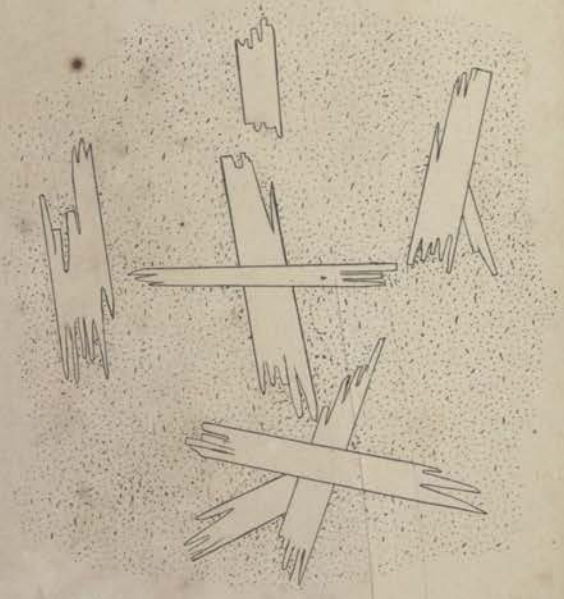

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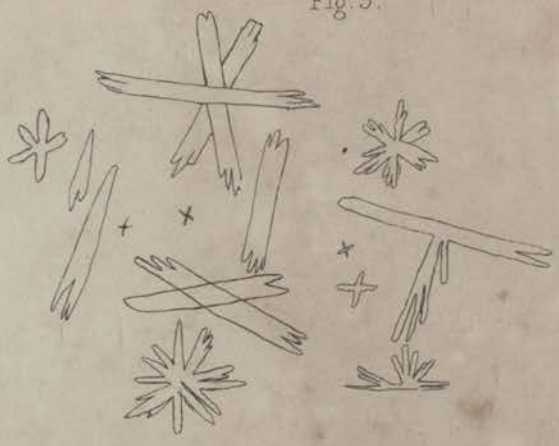

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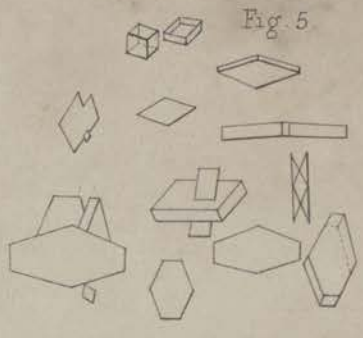

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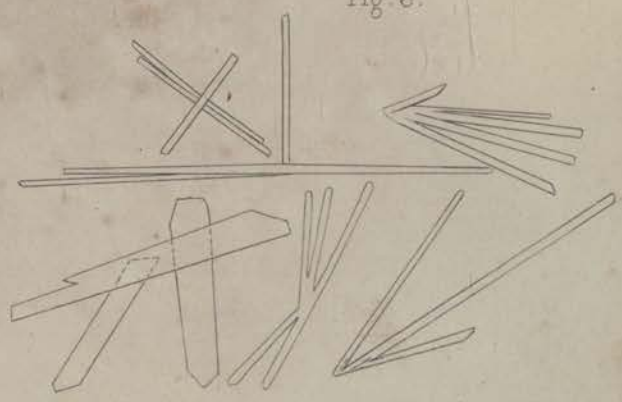

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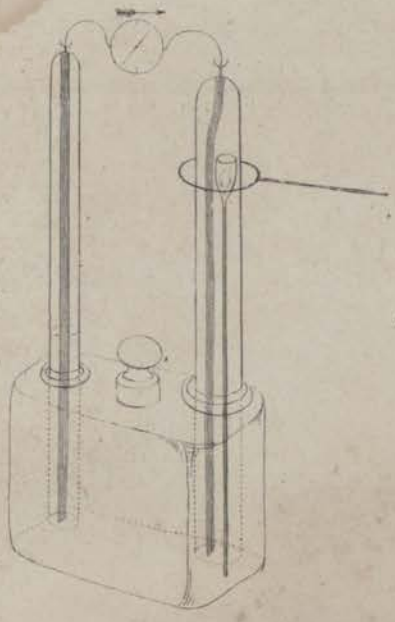

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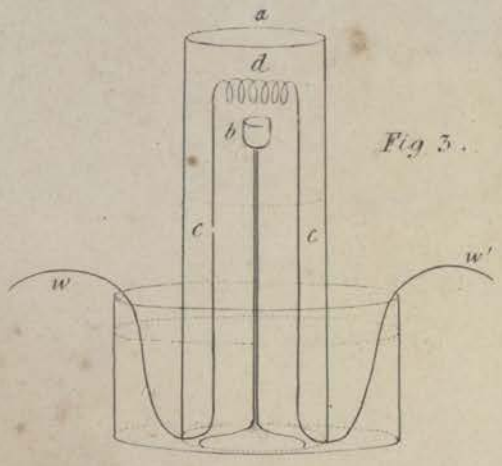

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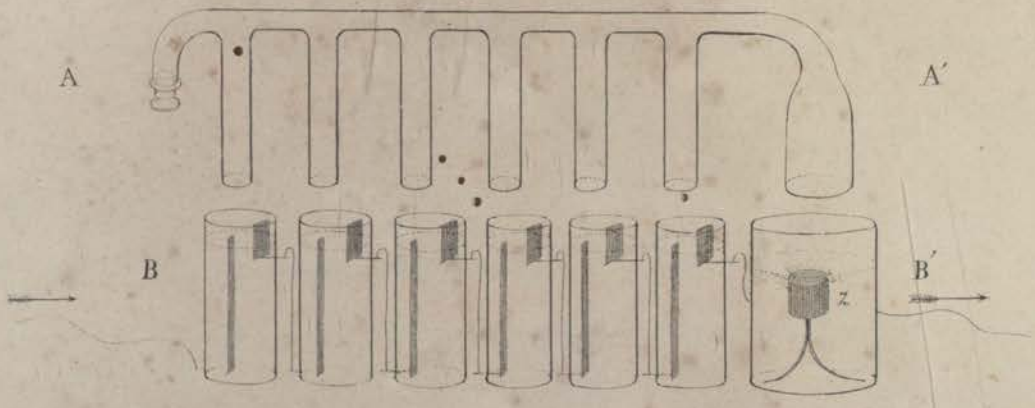

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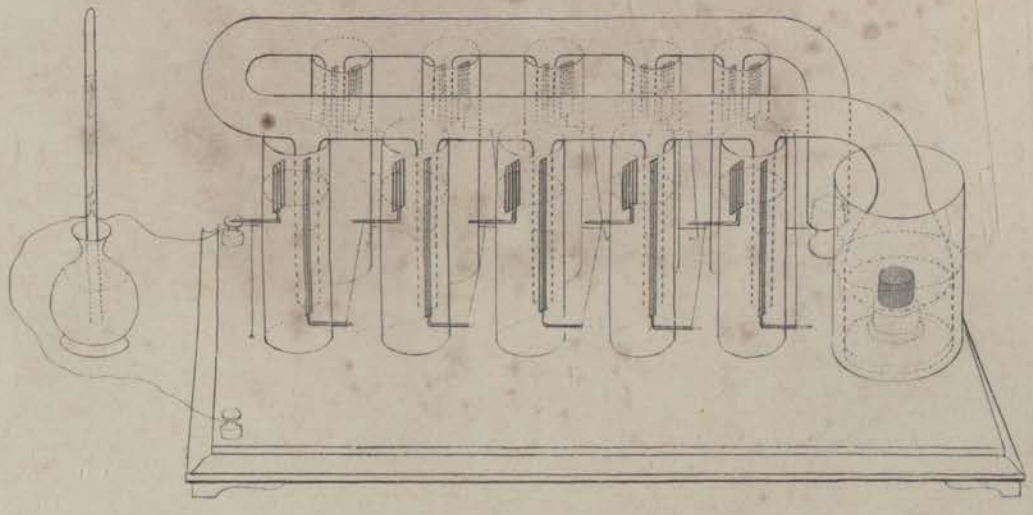

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